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PHYSICAL REVIEW.

THE ATTENUATION OF SOUND AND THE CONSTANT OF RADIATION OF AIR.

By A. Wilmer Duff.

Preliminary.

THE intensity[1] of sound spreading in spherical waves from a source would, if no part of the energy of vibration were lost in the passage, vary inversely as the square of the distance. It is certain, however, that a considerable proportion of the vibrational energy must in every second be converted into heat, though no attempt seems to have been made to determine experimentally how large this portion is. The chief purpose of the present paper is to describe some observations made by the author with a view to obtaining a rough estimate of this important quantity.

While this is probably the first attempt at an experimental investigation, several eminent physicists have discussed the question theoretically. Thus Stokes[2] in 1845 studied the effect of the viscosity of the air and deduced numerical results, and in 1851 he[3] found a formula for the effect of radiation from the condensations and to the rarefactions, but calculation from this formula is still impossible because of our ignorance of the rate of radiation of a gas.[4]

[1] To avoid confusion I shall use "intensity" in the objective sense of flow of energy and loudness in the subjective sense.

[2] Phil. Mag. (4), I., 305.

[3] Cambridge Phil. Trans., VIII., 287.

[4] See remarks by Rayleigh in Nature, No. 1447, Vol. 56 (1897).

Rayleigh [1] has applied Stokes' method to estimate the effect of conduction. All these investigations have referred to plane waves only. Kirchoff [2] in 1868 discussed the effect of both conduction and viscosity on plane waves and indicated the result for spherical waves also. Brunhes [3] has recently examined the effect of conduction on plane waves, obtaining a result in accord with those of Kirchoff and Rayleigh.

In order to deduce any intelligible results from the observations I have made, it is necessary to either assume or establish a law of diminution of intensity in spherical waves, taking account of viscosity, radiation, and conduction simultaneously. As considerable doubt might attach to a formula framed by combining the formulæ already obtained for the separate effects enumerated, it has seemed necessary to give the following brief theoretical discussion of the question.

Theoretical.

The dynamical equations of the *small* motions of a viscous compressible fluid are

$$\left.\begin{aligned}
\frac{\partial u}{\partial t}+\frac{1}{\rho_0}\frac{\partial p}{\partial x}&=\frac{\mu}{\rho_0}\nabla^2 u+\frac{\mu}{3\rho_0}\frac{\partial}{\partial x}\left(\frac{\partial u}{\partial x}+\frac{\partial v}{\partial y}+\frac{\partial w}{\partial z}\right)\\
\frac{\partial v}{\partial t}+\frac{1}{\rho_0}\frac{\partial p}{\partial y}&=\frac{\mu}{\rho_0}\nabla^2 u+\frac{\mu}{3\rho_0}\frac{\partial}{\partial y}\left(\frac{\partial u}{\partial x}+\frac{\partial v}{\partial y}+\frac{\partial w}{\partial z}\right)\\
\frac{\partial w}{\partial t}+\frac{1}{\rho_0}\frac{\partial p}{\partial z}&=\frac{\mu}{\rho_0}\nabla^2 w+\frac{\mu}{3\rho_0}\frac{\partial}{\partial z}\left(\frac{\partial u}{\partial x}+\frac{\partial v}{\partial y}+\frac{\partial w}{\partial z}\right)
\end{aligned}\right\} \qquad (1)$$

where μ denotes the coefficient of viscosity and ρ_0 the normal density. Denoting a condensation by s, the equation of continuity is

$$\frac{\partial u}{\partial x}+\frac{\partial v}{\partial y}+\frac{\partial w}{\partial z}+\frac{\partial s}{\partial t}=0 \qquad (2)$$

This leads to an obvious simplification of (1). The above constitute four equations in five functions of x, y, z, and t, viz.: u, v, w, p, s. A fifth is supplied by the law of gaseous pressure

[1] Theory of Sound (1896), Vol. II., p. 28.

[2] Pogg. Ann., CXXXIV., 289.

[3] Jour. de Phys., VI., p. 289, 1897 (Abstract 473, Phys. Soc. of London, 1897).

$$p = \kappa\rho(1 + a\theta)$$

in which θ denotes *excess* of temperature. But, since ρ denotes the density corresponding to a condensation s,

$$\rho = \rho_0(1 + s),$$

whence
$$p = \kappa\rho_0(1 + s + a\theta) \tag{3}$$

the product of small quantities being neglected.

As this introduces a new function, θ, another equation is needed. This is supplied by the thermal relation

$$\frac{\partial\theta}{\partial t} = \beta\frac{\partial s}{\partial t} + \nu\nabla^2\theta - \lambda\theta \tag{4}$$

in which the first term on the right denotes the rate of change of temperature due to condensation, the second that due to conduction, and the third that due to radiation according to Newton's Law.

To adapt these equations to spherical waves, suppose the origin transferred to the source of sound; multiply (1) by $\frac{x}{r}, \frac{y}{r}, \frac{z}{r}$ respectively and add, and then transform by use of the relation

$$r^2 = x^2 + y^2 + z^2.$$

Denoting the radial velocity by V and writing μ' for μ/ρ, we get

$$\frac{\partial V}{\partial t} + \frac{1}{\rho_0}\frac{\partial p}{\partial r} = \mu'\frac{\partial}{\partial r}\left(\frac{\partial V}{\partial r} + 2\frac{V}{r}\right) - \tfrac{1}{3}\mu'\frac{\partial^2 s}{\partial r\partial t} \tag{5}$$

From (2) and (4) by transformation and substitution of θ' for θ/β we get

$$\frac{\partial V}{\partial r} + 2\frac{V}{r} + \frac{\partial s}{\partial t} = 0 \tag{6}$$

$$\left(\frac{\partial}{\partial t} + \lambda\right)r\theta' - \nu\frac{\partial^2 r\theta'}{\partial r^2} - r\frac{\partial s}{\partial t} = 0. \tag{7}$$

By use of (3) and (6), (5) becomes

$$\frac{\partial V}{\partial t} + \left(\kappa + \tfrac{4}{3}\mu'\frac{\partial}{\partial t}\right)\frac{\partial s}{\partial r} + \kappa a\beta\frac{\partial\theta'}{\partial r} = 0 \tag{8}$$

The motion here considered, being symmetrical about the origin, is necessarily irrotational and a velocity potential φ exists which is a function of r and t only. On introducing it, (6), (7) and (8) become

$$\frac{\partial^2 \cdot r\varphi}{\partial r^2} + \frac{\partial \cdot rs}{\partial t} = 0 \tag{9}$$

$$\left(\frac{\partial}{\partial t} + \lambda\right) r\theta' - \nu \frac{\partial^2 \cdot r\theta'}{\partial r^2} + \frac{\partial^2 \cdot r\varphi}{\partial r^2} = 0 \tag{10}$$

$$\frac{\partial \varphi}{\partial t} + \left(\kappa + \tfrac{4}{3}\mu' \frac{\partial}{\partial t}\right) s + \kappa a_1 \beta \theta' = 0 \tag{11}$$

An arbitrary function of t has been omitted from (11) as it may be supposed to be incorporated in φ and to disappear on the restoration of V.

These equations being linear and the motion known to be periodic, we may assume that φ, s, θ' contain t only in virtue of the factor ε^{ht}, h being a constant which will afterwards be regarded as imaginary. Hence

$$\frac{\partial^2 \cdot r\varphi}{\partial r^2} + hrs = 0 \tag{12}$$

$$(h + \lambda) r\theta' - \nu \frac{\partial^2 \cdot r\theta'}{\partial r^2} + \frac{\partial^2 \cdot r\varphi}{\partial r^2} = 0 \tag{13}$$

$$hr\varphi + (\kappa + \tfrac{4}{3} h\mu') rs + \kappa a_1 \beta r\theta' = 0 \tag{14}$$

Eliminating rs and $r\theta'$ and putting $\gamma = 1 + a_1\beta$ we get

$$A \frac{\partial^4 \cdot r\varphi}{\partial r^4} - B \frac{\partial^2 \cdot r\varphi}{\partial r^2} + C \cdot r\varphi = 0 \tag{15}$$

where

$$A = \frac{\nu}{h} (\kappa + \tfrac{4}{3} h\mu')$$

$$B = \kappa\gamma + h(\tfrac{4}{3}\mu' + \nu) + \frac{\lambda}{h} (\kappa + \tfrac{4}{3} h\mu')$$

$$C = h^2 \left(1 + \frac{\lambda}{h}\right)$$

Hence the required solution is $r\varphi = \varepsilon^{ht} \cdot \varepsilon^{qr}$, where q satisfies the equation

$$Aq^4 - Bq^2 + C = 0 \tag{16}$$

Now for air at 0°C. and atmospheric pressure the value of μ' is .132 and of ν .2567.[1] Hence μ' and ν may be regarded as small quantities. Our knowledge of the value of λ is extremely vague. For notes of very low pitch λ may be comparable in magnitude with the real part of h; but for the very high-pitched notes employed in the second part of this paper (for which the real part of $h = 2\pi \times 7000$) it is evident that λ cannot be comparable with the real part of h; for if it were, a mass of air initially at any excess of temperature above the surrounding mass would fall, by radiation only, to one-half of that excess in a length of time comparable with one sixty-thousandth of a second. Hence we shall first solve (16) for the case treated experimentally. This will enable us to make a rough estimate of the value of λ and it will then be seen that the solution will hold for all sounds except those of the very lowest pitch.

Hence for the first approximation to q we neglect μ', ν and λ/h. Replacing h by ni we thus obtain

$$q = \pm \frac{n i}{\sqrt{k \gamma}}$$

To obtain the second approximation let

$$q = \pm \left(\frac{n i}{\sqrt{k \gamma}} + m \right) \tag{17}$$

and substitute, neglecting products of μ', ν, λ/h and m. We thus get

$$m = \frac{n^2}{\sqrt{k \gamma}} \left(\tfrac{4}{3} \mu' + \frac{\gamma - 1}{\gamma} \nu \right) + \frac{\gamma - 1}{\gamma} \frac{\lambda}{\sqrt{k \gamma}} \tag{18}$$

Thus the typical solution for $r\varphi$ is

$$r\varphi = \varepsilon^{-mr} \varepsilon^{ni(t - r/a)} \tag{19}$$

in which $a = \sqrt{k \gamma}$ = the velocity of sound and the lower sign in (17) has been chosen as corresponding to the outward traveling wave of diminishing intensity. An abitrary multiplier may be introduced and an arbitrary constant added to t. Omitting these and

[1] The absolute conductivity is taken as .00005572 as obtained by E. Müller, Wied. Ann., 60, 113 (1896).

realizing the solution by discarding the imaginary part of the above expression we get for V

$$V = \frac{\partial}{\partial r}\left[\frac{\varepsilon^{-mr}}{r}\cos n\left(t-\frac{r}{a}\right)\right]$$

$$= \varepsilon^{-mr}\left[\frac{n}{ar}\sin n\left(t-\frac{r}{a}\right) - \frac{1+mr}{r^2}\cos n\left(t-\frac{r}{a}\right)\right]$$

Thus the motion at any point consists in the superposition of two simple harmonic vibrations differing in phase by a quarter of a period. The ratio of the amplitude of the second to that of the first is

$$m+\frac{1}{r} \;:\; \frac{n}{a}$$

Now $\frac{n}{a}$ is the real part of the first approximation to q and m is the difference between the first and second approximations; also $1/r$ rapidly diminishes as r increases. Hence, after a short distance the motion is sensibly

$$V = \frac{\varepsilon^{-mr}}{r}\sin n\left(t-\frac{r}{a}\right) \qquad (20)$$

Hence the intensity varies as

$$\frac{\varepsilon^{-2mr}}{r^2}.$$

It will be seen from (18) that m consists of three parts due to viscosity, conduction, and radiation respectively and that, while the first two increase very rapidly with increase of the vibration frequency, the third, due to radiation, is independent of the vibration frequency.

Experimental.

It is well known that great difficulties are encountered in any attempt to measure the intensity of sound or study its variations. In the following method the rate of decay of intensity is deduced from the distances at which sounds of different intrinsic intensity become inaudible and it is assumed that *when two faint sounds are indis-*

tinguishable by the ear as regards pitch and quality the minimum intensity required for audibility is the same for both.

A large number of very small whistles were made of as nearly as possible the same shape and dimensions. From these the eight that seemed most similar in all respects were chosen and mounted on a wind-chest in such a way that any number could be blown under a definite pressure indicated by a water manometer. The distances at which each pair and all the eight just became inaudible were then determined under conditions to be described presently. Let R be the distance at which all eight whistles blown at once became inaudible and r the mean distance at which two became inaudible. Then at a distance r the mean intensity of two whistles equalled the minimum for audibility and hence that of the eight whistles equalled four times [1] that minimum, while at distance R the intensity of the eight whistles just equalled the minimum for audibility. Now it was found that near the limits of audibility the sounds were indistinguishable in quality. Hence, assuming (as stated above) that the minimum was the same in both cases, we have by the preceding theory

$$\frac{\varepsilon^{-2mR}}{\varepsilon^{-2mr}} \cdot \frac{r^2}{R^2} = \frac{1}{4}$$

and

$$m = \frac{\log_e \left(2 \frac{r}{R} \right)}{R - r}$$

The observations were made at a very quiet place on the River St. John in New Brunswick, Canada, the whistles being sounded on

[1] It is here tacitly assumed that the intensity of the eight whistles equals the sum of their intensities when sounded separately. This may (if justification be needed) be justified on two grounds: Firstly, it was ascertained that the distance apart of the whistles on the wind-chest had no effect on the distance of audibility and that the quantity of energy spent in sounding all eight whistles (as measured by the wind expenditure at constant pressure) was the sum of the quantities expended in sounding them separately. Hence, the total energy of vibration produced is the sum of the quantities which would be produced by separate blowing. Secondly, that the intensities at any distance are also additive may be justified from the analysis. For in the case of the coexistence of several notes the solution for V will be the sum of such partial solutions as (20), and by integrating $V \triangle p$ through a large multiple of the mean period it can be shown that the complex sound follows the same law of attenuation as the simple sounds (m being the mean of the values for the separate sources) and that the resultant mean intensity is the sum of the separate intensities.

one side of the river and the sounds listened to on the other side. Only times when there was no appreciable wind were considered suitable for work. To eliminate the effect of reflection from the banks several different stations for sounding and directions for observing were tried in different sets of observations. The orifices of the whistles were always kept turned directly toward the observer. To avoid errors due to tiring of the ear in some cases the observations on the pairs of whistles were made first and in other cases those on the eight whistles. Various devices were tried to eliminate the effect of bias in determining the distance of inaudibility. The difficulty of determining the point at which the sound became inaudible was found much less than was expected. Nevertheless, there was always a space in which it was doubtful whether the sound was heard or imagined. The middle of such a space was taken as the most probable position of extinction.

The relative intensities of the different pairs of whistles may be judged from the fact that in one case the distances of inaudibility were 508, 511, 518, 529 meters and in another 622, 629, 636, 647 meters. These were the only cases in which the distance for each separate pair was finally measured. In general, the position for each pair was marked by a stake and the mean distance finally taken for measurement. In the absence of facilities for the purpose the pitch of the whistles was not determined until some weeks after the experiments were made. It was then found by using a high pressure sensitive flame to locate the nodes of the stationary waves produced by reflection from a wall. The semi-wave-lengths thus found at 18° C. were 2.49, 2.46, 2.41, 2.41, 2.46, 2.42, 2.42, 2.44 cms. The mean of these is 2.44, corresponding to a vibration frequency of 7000.

In the following table of results the C. G. S. system of units is employed:

	r	R	t	m
1	38400	50600	27° C.	.000034
2	40700	51800	23	.000040
3	63400	75400	15	.000043
4	45800	55900	26	.000049
5	59700	71200	15	.000045
			Mean	.000042

It may be noted that No. 1 was a preliminary experiment made with a view to finding the best conditions of work and was completed although a slight wind had sprung up which interfered with the observations. This probably accounts for the exceptional value of m. The very great differences in the values of r and of R on different occasions were due chiefly to wide variations in disturbing sounds such as those produced by birds and insects, the rustling of grass and the ripple of water.

It will be seen from this table that the eight whistles were, on an average, audible about one-fourth farther than a pair of whistles, whereas, if the sphericity of the waves had been the only cause of attenuation, the distance would have been as two to one. It will also be seen from the value of $2\ m$ that the total energy of vibration decreases at about five-sixths of one per cent. per meter.

From (18) it can readily be calculated that for the high pitched notes employed the part of m due to viscosity is .0000050, and the part due to conduction .0000019. The difference between the sum of these and the value for m found experimentally is .000035. It will thus be seen what a relatively small part of the whole effect is due to viscosity and conduction, and what a large part is due to radiation and whatever other causes have not been considered. Of such hitherto unconsidered causes the only two that readily suggest themselves are atmospheric refraction (which causes a curvature of the lines of radiation) and reflections arising from a lack of homogeneity of the medium.

It does not, however, seem probable that either of these causes has contributed largely to the value of m. For, while viscosity, conduction, and radiation are comparatively constant in their action, refraction and internal reflection are causes which must vary very widely with circumstances of position and of the condition of the atmosphere and, had they been accountable for any considerable part of the effect observed, the separate values of m could not have shown such a fair degree of accordance as they do. To make this clearer in regard to refraction, it may be noted that (1) there was practically no wind (the chief source of refraction), (2) the vertical temperature gradient (the only other considerable source of refraction) must have varied widely, since the time of observation varied

from the heat of noonday to the cool of evening after sunset (in the last observation), (3) the height of the station at which the whistles were blown varied from three feet to nine feet above the level of the water, while the height of the observer's ear varied from six feet to ten feet.

Again, whatever the circumstances are that determine that lack of uniformity or "flocculence" of the atmosphere which was supposed by Tyndall to produce internal reflection, it seems highly probable that they varied widely because of (1) the variation of time of observation and of temperature already noted and (2) variations in the hygrometric state of the air ranging from comparative dryness to (in one case) saturation before a heavy shower, and as the values of m show no such wide variations it seems improbable that much of the diminution of intensity was due to this cause. It would seem highly probable that, in any case, the air above a large body of water on a still day is comparatively uniform. No continuance of the sound after blowing had ceased could be observed, which is contrary to what would be expected if there had been much internal reflection. It may be noted also that internal reflections do not themselves produce a diminution of intensity in the case of a sustained note, for the reflection does not involve a diminution of energy of vibration, and reflections toward the observer will compensate for reflections away from him. Nevertheless the prolonging of the time of transmission from the source to the observer may lead to an increase in the effects due to viscosity, conduction, and radiation.

Assuming then that refraction and reflection contributed but little to the diminution of intensity observed, we see that radiation is about seven times as effective as viscosity and about eighteen times as effective as conduction in reducing the intensity of a note whose vibration frequency is 7000. But the effects of viscosity and conduction are proportional to the square of the frequency, while that of radiation is the same for all sounds. Hence for a note of medium pitch, for example one whose vibration frequency is 350, radiation must far transcend viscosity and conduction in effect, being in fact nearly 3000 times as effective as viscosity and over 7000 times as effective as conduction. The whole rate of diminution would, so far as these three causes are concerned, be practically that of radiation alone or about two-thirds of one per cent. per meter.

It seems, therefore, improbable that, as some have thought, the mellowing of harsh sounds with distance is due to a higher rate of decay of the higher components. It is more probably due to their smaller initial energy which causes them, even with nearly the same rate of decay, to drop sooner to the limits of audibility.

The Constant of Radiation of Air.

According to Newton's law, the rate of cooling of a gas by radiation bears a constant ratio to the excess of its temperature above that of its surroundings. From the preceding experimental estimate of the effect of radiation on sound intensity and the theoretical value as given by equation (18), it can be calculated that the value of this constant is for air about 8.3. It follows that a mass of air at any given excess of temperature above its surroundings will, if its volume remains constant, fall by radiation to one-half of that excess in about one-twelfth of a second. For reasons that appear in the preceding section this estimate of the rate of radiation must be regarded as more probably too great than too small.

Large as this estimate of the constant of radiation may seem to be, it is still not inconsistent with the known fact that the velocity of sound is (within the limits of experimental error) independent of the pitch. This can be seen by integrating (15) on the assumption that λ is not negligible compared with n or by calculation from the results of Stokes' discussion of the effect of radiation. Even for a note of as low frequency as 50, it would only produce a decrease of velocity of one part in three thousand.

There is another respect in which the somewhat large value obtained for the constant of radiation is of interest. It would seem to point to an intimate association between gaseous particles and the ether, permitting of a rapid transfer of energy of vibration from the former to the latter.

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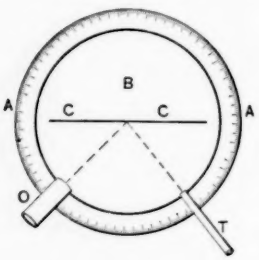
MINOR CONTRIBUTIONS.

ON THE POLARIZATION OF LIGHT REFLECTED FROM HARD RUBBER.

BY ALICE H. BRUÈRE.

IF the light reflected from polished black rubber or ebonite is observed through a Nicol prism it is found to show unmistakable evidences of polarization. At an angle of incidence of about fifty degrees the polarization appears to be as complete as that obtained by reflection from glass. This fact suggests the possibility of using hard rubber in place of black glass in the various forms of polarizing apparatus. The question also arises as to whether the polarization by reflection from an opaque substance, such as ebonite, obeys laws similar to those found in the case of a transparent material. The following observations were therefore taken to determine the dependence of the amount of polarization upon the angle of incidence.[1] The apparatus was arranged as follows:

The prisms were removed from the table of a Browning spectrometer. In place of the observation telescope was placed a tube—*O* (see Fig. 1),



A = table with graduated circle.
B = spectrometer table.
C = rubber plate.
O = observation tube.
T = collimator.

Fig. 1.

which will be designated the observation tube. In the construction of this tube the arrangement used by R. A. Millikan[2] in his study of the polarization of the light emitted by incandescent solid and liquid surfaces, was closely followed. Its parts are shown in Fig. 2.

[1] The investigation was suggested by Professor Merritt, and during the work much valuable assistance was given by his suggestions and criticism.

[2] PHYSICAL REVIEW, Vol. III., p. 81, 1895.

A = aperture, 2 × 3 mm., cut in a metal plate.
R = Iceland spar rhomb, having its axis perpendicular to the spectrometer table.
N = Nicol prism. L = eye-piece. A' = aperture.
K = graduated circle on brass tube.
X = index by which position of prism was read.
Length of observation tube, 10.5 cm.

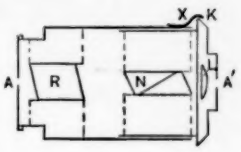


Fig. 2.

The Iceland spar rhomb was firmly set in cork and fixed in the tube.

The Nicol prism and eyepiece were fixed in a short brass cylinder, to which the graduated circle was also attached. This cylinder rotated within the tube.

The plate of hard rubber was placed on the spectrometer table against a support, so arranged that the front surface of the plate was on a diameter of the table, therefore on a diameter of the graduated circle of the spectrometer, and perpendicular to the table.

The source of light was a sodium flame placed at the end of the collimator, from which the cap containing the slit had been removed. The light, therefore, consisted of approximately parallel rays. The extent of surface illuminated was limited by a cap placed over the lens of the collimator, in whose center was cut an aperture 6 mm. in diameter.

The positions of observation tube and collimator were read from the graduated circle of the spectrometer. The apertures before the lens of the collimator and before the Iceland spar rhomb of the observation tube were placed at the centers of the tubes. The tubes were adjusted with their centers in line with a radius of the table, by means of adjusting screws on their supports.

Since the tubes were set with reference to the perpendicular, it was necessary to accurately determine the perpendicular to the surface of the plate under examination. For this purpose the plates, when placed on the table, were adjusted with their vertical edges at equal distances from the circumference. For the larger plates, whose edges were within two centimeters of the circumference, the observation tube was brought into that position in which the edge of the plate could be seen as a line through the centers of the images, and the angle was then read from the graduated circle of the spectrometer. This was done for each edge. For the smaller plates a straight edge was accurately adjusted in line with the surface and near the circumference, and the positions of this edge were then determined as before. In all cases the readings for opposite edges differed by approximately 180°, the error being not greater than 0°.5.

With collimator and observation tubes set at a definite angle, a beam of light was sent to the plate and reflected to the aperture before the Iceland spar rhomb of the observation tube. Here it was doubly reflected

by the rhomb and transmitted to the Nicol prism. After transmission through this, two images of the aperture could be seen in the eye-piece.

If the beam of light reflected from the plate remained unchanged as to direction of vibration, two beams of equal intensities, polarized at right angles to each other, would be transmitted by the rhomb. Each of these beams would be extinguished when the principal plane of the Nicol was parallel to its plane of polarization, and half way between these points the intensities of the images would be equal. If, however, the original beam was partially polarized by reflection from the plate, the position of equal intensities would be nearer the point of extinction for the more intense beam.

The principal section of the Iceland spar rhomb was set perpendicular to the plane of polarization. This was done in the following way: The spectrometer table was made horizontal; on this was placed a small wooden support from which a slender thread, with a weight attached at one end, could be dropped as a plumb-line. This thread was near the small aperture. Across the aperture, and in line with the thread, a fine quartz fiber was then firmly fastened. The rhomb, which was set in cork, was then adjusted until only one image of the fiber could be seen through it. In this position the axis of the crystal was parallel to the fiber. Since the latter had been placed perpendicular to the spectrometer table, the axis of the crystal was therefore parallel to the surface of the rubber plate.

Since the plane of polarization of the reflected beam was perpendicular to the plate, that position of the Nicol which made the intensities of the transmitted images equal defined the relation between the original intensities of the two beams. The light which formed these images was, as before stated, approximately parallel light. If the reflecting surface had been perfectly plane, all rays would have been reflected at the same angle, and the determination of the amount of polarization for a given angle would have been correct. But the surfaces of the rubber plates were not exactly plane, and the angle of reflection of the rays may therefore have varied. The aperture before the rhomb was 2 x 3 mm.; the extent of surface from which light for the images was received was carefully measured in several sets of observations. The measurements for the different plates agreed closely, and the surface increased gradually from a space 0.2 inches wide for an angle of 17°, to a space one-half inch wide for an angle of 82°. The radius of the table was 9.5 cm. The error introduced by a change in the direction of the reflected rays which may form one image, can, therefore, be only a small one. The character of a particular surface would determine whether the change would be an increase or a decrease in the angle, and the final result might, in consequence, be either too great or too small.

The formula used by R. A. Millikan also applies here, and the degree of polarization was calculated by it.

Let a and b represent the original amplitudes of vibration in the two beams. The intensities of the beams are then represented by a^2 and b^2. The proportional amount of polarization, if a^2 represent the greater intensity, is then

$$\frac{a^2 - b^2}{a^2 + b^2}$$

Let p represent the degree of polarization in the original beam, then

$$p = \frac{a^2 - b^2}{a^2 + b^2}. \qquad (1)$$

If w is the angle which the transmitting plane of the Nicol makes with the direction of vibration of the more intense beam, a^2, the intensities of the images as seen through the Nicol will, by the law of Malus, be $a^2 \cos^2 w$ and $b^2 \sin^2 w$.

For the position of equality this would be,

$$a^2 \cos^2 w = b^2 \sin^2 w. \qquad (2)$$

$$\frac{a^2}{b^2} = \frac{\sin^2 w}{\cos^2 w}. \qquad (3)$$

Combining (1) and (3) we have,

$$p = \frac{\sin^2 w - \cos^2 w}{\sin^2 w + \cos^2 w} =$$

$$- \frac{\cos^2 w - \sin^2 w}{1} = - \cos 2w. \qquad (4)$$

Cos 2 w then determines the degree of polarization in the original beam, when the position of equal intensities for the two images has been found. The Nicol was therefore set for equality on both sides of a point of extinction for the less intense image.

Three plates were examined. Plates I. and II. were each 13 cm. x 15 cm., and 1.5 mm. thick. Plate III. was 7 mm. thick. The polish of Plates II. and III. was much higher than that of Plate I.

Because the angle of polarization was found to be nearly the same as that for glass, observations similar to those for the rubber plates were taken for a glass plate. A piece of plane-parallel glass made by J. A. Brashear was used.

For Plate I. and Plate II. four sets of observations were taken. For Plate III. and for the glass plate three sets were taken. The results obtained with Plate II. for two angles are given in Tables I. and II. below. The first of these shows the greatest variation, the second shows the closest agreement obtained in the settings for equality of the images.

When the several averages for settings made for a given angle at one time are considered, few vary more than two degrees, and many vary only one degree. In Table III. are given the averages for all four plates studied.

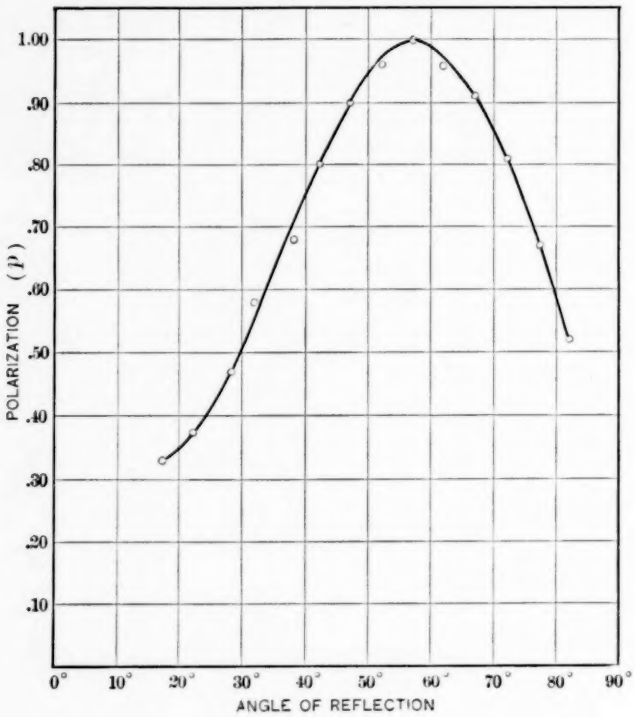


Fig. 3. Rubber. I.

TABLE I.

RUBBER PLATE No. II.

Angle of Reflection = 27°.

Nicol Scale.	*L.*	*R.*	*L.*	*R.*	*L.*	*R.*	*L.*	*R.*
	173	52	176	51	174	52	174	51
	171	52	173	51	173	52	175	51
	172	52	173	51	174	53	174	51
	172	53	173	52	174	51	176	52
	171	51	174	52	175	51	175	51
	111	51	174	51	174	52	174	51
Average.	172	52	174	51	174	52	175	51

General Average, 173.7 — 51.5 $2w = 122°.2$.

TABLE II.

RUBBER PLATE No. II.

Angle of Reflection = 62°.

Nicol Scale.	*L.*	*R.*	*L.*	*R.*	*L.*	*R.*	*L.*	*R.*
	196	26	197	26	196	27	196	26
	196	26	197	26	196	27	195	26
	196	26	197	25	196	27	196	26
	196	26	197	26	196	27	196	26
	196	26	197	26	196	27	196	26
	196	26	196	26	196	27	196	26
Average.	196	26	197	26	196	27	196	26

General Average, 196.2 — 26.2 $2w = 170°$.

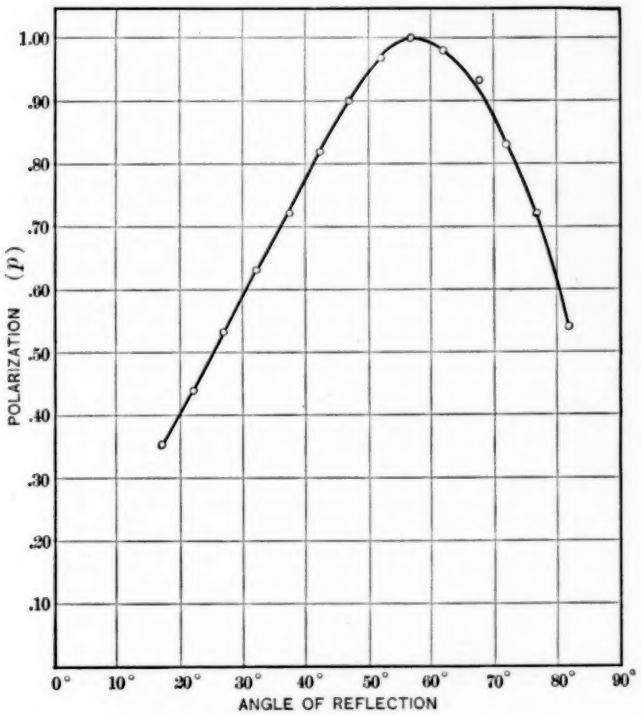


Fig. 4. Rubber, II.

From the average of the results obtained with each plate, curves were drawn, in which the angles of reflection were used as abscissas, and the

cosines of double the angles which the transmitting plane of the Nicol made with the direction of vibration of the more intense beam, were used as ordinates. These curves are shown in Figures 3, 4, 5, and 6. In Fig. 7 all four curves are plotted for comparison.

The curves for Plates II. and III. agree closely. Accepting these as more nearly correct than the one for Plate I., the true point of total polarization of light by reflection from hard rubber would be 58°+. The excess is within ¼°.

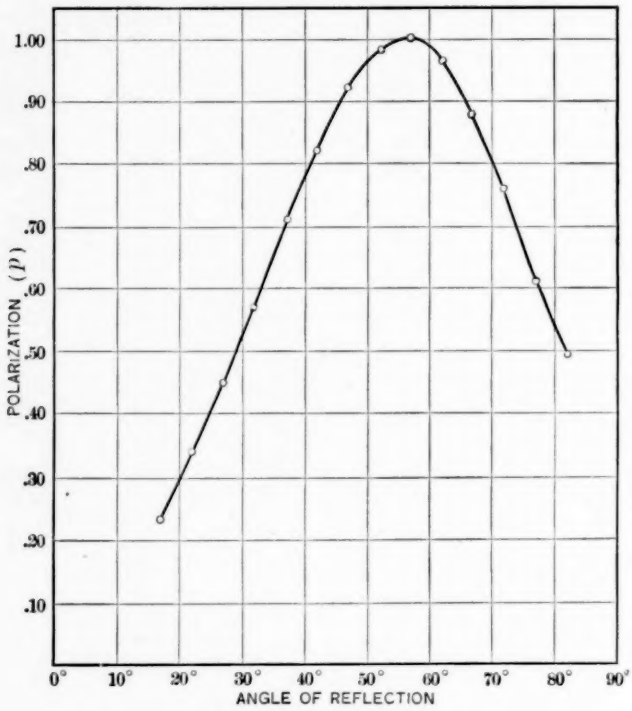


Fig. 5. Rubber, III.

Using the curve derived for the glass plate in the same way, the angle for total polarization by reflection from glass is 56°—. The deficiency is within ¼°. Preston gives the angle as "about 57°," and Spottiswoode gives it as 54° 35′. Since the polarizing angle would vary with a change in the index of refraction of the glass, the result obtained by the experiment may be considered approximately correct.

By Brewster's law, the index of refraction of a substance is the tangent of the angle of polarization; *i. e.*, tan $i = \mu$.

Applying this to the result obtained for the rubber plates, tan $58° = \mu$ for sodium light.

$$\tan 58° = 1.60$$
$$\tan 58° 15' = 1.61.$$

The index of refraction of hard rubber would then be not less than 1.60, not greater than 1.61, for sodium light.

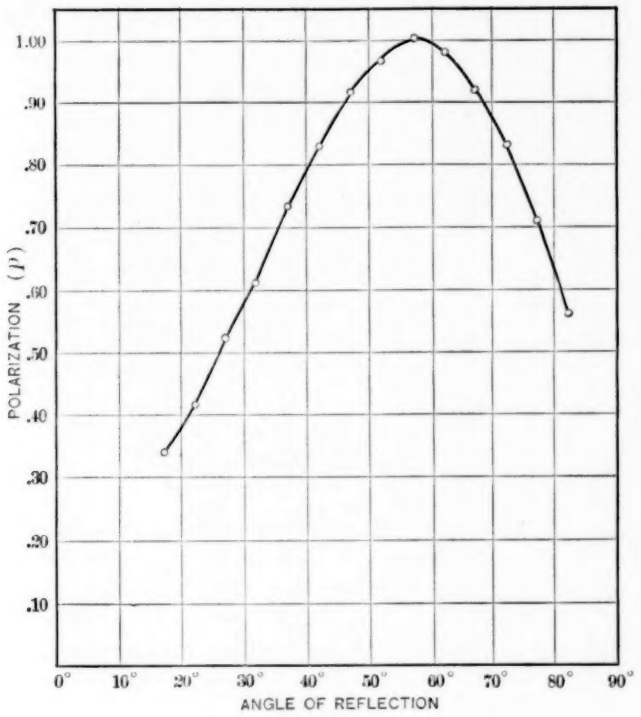


Fig. 6. Glass.

These results do not agree with those obtained by Jamin, who found that only a few substances completely polarize light by reflection, and that for these $\mu = 1.46$.

The curve for the glass plate, if extended, would reach the origin without a change of direction, and therefore indicates that there is no polarization for normal incidence of the beam of light. The curves for the rubber plates cannot reach the origin without a change of direction, and would therefore indicate a small degree of polarization even for normal incidence, which is hardly possible. The only difference in the conditions of the experiment in the two cases, so far as known, was a difference

TABLE III.

Angle of Reflection.	Plate I.		Plate II.		Plate III.		Glass Plate.	
	$2w$	cos $2w$ $=p$.	$2w$	cos $2w$ $=p$.	$2w$	cos $2w$ $=p$.	$2w$	cos $2w$ $=p$.
17°	109.°7	.33	110.°5	.35	110.°3	.34	103.°3	.23
22°	111. 8	.37	116. 5	.44	115.	.42	110.	.34
27°	118. 3	.47	122. 2	.53	122.	.52	117.	.45
32°	126.	.58	129. 5	.63	128.	.61	125.	.57
37°	133. 5	.68	136. 5	.72	136.	.73	135. 7	.71
42°	144.	.80	145. 5	.82	146.	.83	146.	.82
47°	154. 8	.90	155. 5	.90	156.	.92	157.	.92
52°	164. 7	.96	166.	.97	166. 7	.97	169. 6	.98
57°	180.	1.00	180.	1.00	180.	1.00	180.	1.00
62°	164. 7	.96	170.	.98	169. 7	.98	164. 4	.96
67°	156. 3	.91	159. 5	.93	158. 3	.92	152. 4	.88
72°	144. 3	.81	146. 5	.83	146. 3	.83	140.	.76
77°	132. 5	.67	136. 7	.72	135. 4	.71	128.	.61
82°	121. 5	.52	123.	.54	124. 4	.56	119. 6	.49

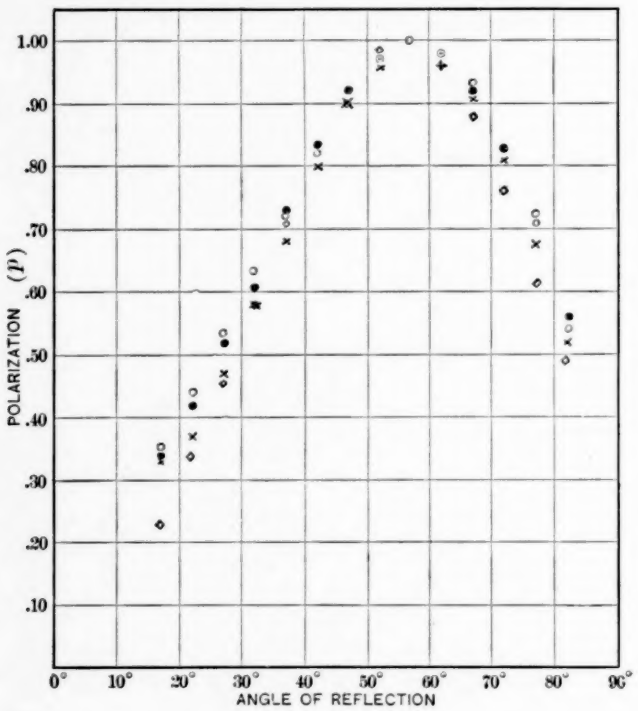


Fig. 7. Rubber and Glass.

in the smoothness of the surfaces. The polarization of the beams reflected at small angles was probably affected by a change in the angle at which the beam fell upon the Iceland spar rhomb. However, it would be necessary to test this point by further experiments before a conclusion could be reached.

For comparison, and as a test of the results obtained with the glass plate, the relative amount of polarization for light reflected at the angles used in the experiment was computed by Fresnel's formula for the intensity of reflected light.

Fresnel's expression is:

$$I = \tfrac{1}{2} a^2 \left[\frac{\sin^2 (i - r)}{\sin^2 (i + r)} + \frac{\tan^2 (i - r)}{\tan^2 (i + r)} \right],^1$$

in which the first term within the bracket represents the intensity of the light polarized in the plane of incidence, while the second term represents that of the light polarized perpendicularly to the plane of incidence. i is known, and u has been determined by experiment. By simple transformations we can change the above expression to one involving only these quantities.

$$\frac{\sin (i - r)}{\sin (i + r)} = \frac{\mu - \sqrt{1 - \sin^2 i}}{\mu + \sqrt{1 - \frac{\sin^2 i}{\mu^2}}}. \qquad (1)$$

$$\frac{\tan (i - r)}{\tan (i + r)} = \frac{\mu - \sqrt{1 - \sin^2 i}}{\mu + \sqrt{1 - \frac{\sin^2 i}{\mu^2}}} \cdot \frac{\cos i \sqrt{\mu^2 - \sin^2 i} - \sin^2 i}{\cos i \sqrt{\mu^2 - \sin^2 i} + \sin^2 i}. \qquad (2)$$

Let α and β represent the two fractions whose product appears in the right hand member of equation (2); then Fresnel's expression becomes:

$$I = \tfrac{1}{2} a^2 \left[\alpha^2 + \alpha^2 \cdot \beta^2 \right].$$

The relative amount of polarization in the reflected beam, *i. e.*, "p" of the curves, will be $\dfrac{\alpha^2 - \alpha^2 \cdot \beta^2}{\alpha^2 + \alpha^2 \cdot \beta^2}$.

For glass $\mu = \tan 56° = 1.48$ was used; for rubber $\mu = \tan 58° = 1.60$ was used.

[1] The Theory of Light, Preston, p. 290.

The results of computation[1] are given in Tables IV and V, the former referring to glass and the latter to rubber.

TABLE IV. GLASS.

POLARIZATION BY REFLECTION WHEN $\mu = 1.48$.

i	a	β	a^2	$a^2\,\beta^2$	$a^2 - a^2\,\beta^2$	$a^2 + a^2\,\beta^2$	$\frac{a^2 - a^2\,\beta^2}{a^2 + a^2\,\beta^2}$
17°	.208	.88	.0432	.0334	.0098	.0766	.127
22°	.213	.81	.0453	.0295	.0158	.0748	.211
27°	.228	.72	.0519	.0268	.0251	.0787	.318
32°	.238	.62	.0566	.0216	.0350	.0782	.447
37°	.259	.49	.0670	.0158	.0512	.0828	.618
42°	.291	.38	.0846	.0121	.0725	.0967	.749
47°	.309	.24	.0954	.0054	.0900	.1008	.892
52°	.333	.12	.1108	.0015	.1093	.1123	.973
57°	.376	−.02	.1413	.00005	.1412	.1413	1.00
62°	.429	−.17	.1840	.0053	.1787	.1893	.944
67°	.494	−.30	.2440	.0219	.2221	.2659	.835
72°	.566	−.45	.3203	.0648	.2555	.3851	.663
77°	.644	−.58	.4147	.1395	.2752	.5542	.496
82°	.772	−.74	.5959	.3263	.2696	.9222	.292

TABLE V. RUBBER.

POLARIZATION BY REFLECTION WHEN $\mu = 1.60$.

i	a	β	a^2	$a^2\,\beta^2$	$a^2 - a^2\,\beta^2$	$a^2 + a^2\,\beta^2$	$\frac{a^2 - a^2\,\beta^2}{a^2 + a^2\,\beta^2}$
17°	.245	.89	.0600	.0474	.0126	.1074	.117
22°	.254	.82	.0645	.0433	.0212	.1078	.196
27°	.264	.74	.0697	.0381	.0316	.1078	.293
32°	.280	.65	.0784	.0331	.0453	.1115	.406
37°	.300	.53	.0900	.0252	.0648	.1152	.562
42°	.322	.42	.1036	.0182	.0854	.1218	.701
47°	.350	.29	.1225	.0103	.1122	.1328	.844
52°	.385	.16	.1482	.0037	.1445	.1519	.950
57°	.422	.03	.1780	.0001	.1779	.1781	.998
62°	.474	−.11	.2246	.0027	.2219	.2273	.976
67°	.538	−.24	.2894	.0166	.2728	.3060	.891
72°	.608	−.40	.3696	.0591	.3105	.4287	.724
77°	.684	−.54	.4678	.1364	.3314	.6042	.548
82°	.797	−.71	.6352	.3202	.3150	.9554	.329

[1] In computing these tables the values of sin i and cos i were carried only to two significant figures.

The results contained in Tables IV and V are shown graphically in Figures 8 and 9.

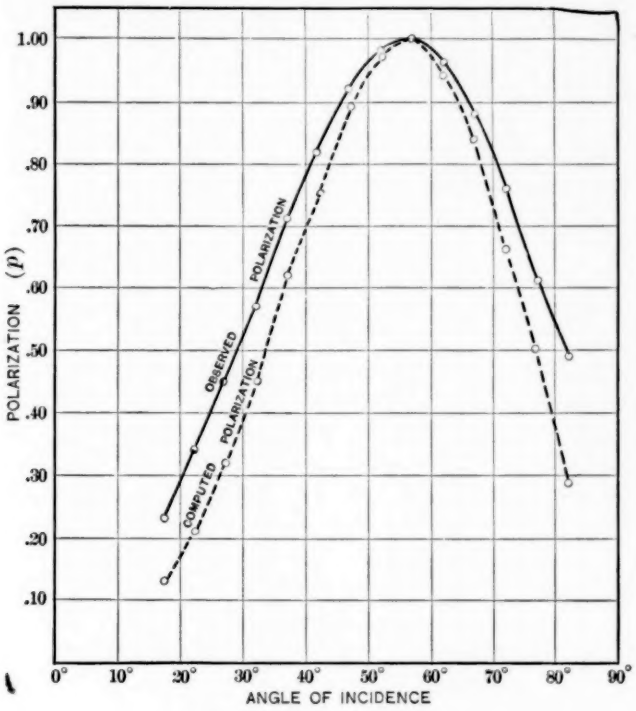


Fig. 8. Glass.

The curve for glass derived by experiment agrees closely with that by the formula. The general character of the curves is the same, though the value for the amount of polarization for each angle is somewhat greater when obtained by experiment than when obtained by theory. It is possible that the lack of complete agreement between the two curves is in part due to reflection from the second surface. But if multiple reflection occurs theory shows that complete polarization is not to be expected at *any* angle of incidence. The fact that the polarization was practically complete at an incidence of 57° may thus be taken as an indication that the error due to multiple reflection is small. In the curves for hard rubber, there is a difference in the character of that part of the curve which represents the polarization at small angles. One indicates polarization for normal incidence, the other indicates no polariaztion for normal incidence, nor for angles less than 5°. The difference in the amount of polarization at the various angles is also greater than in

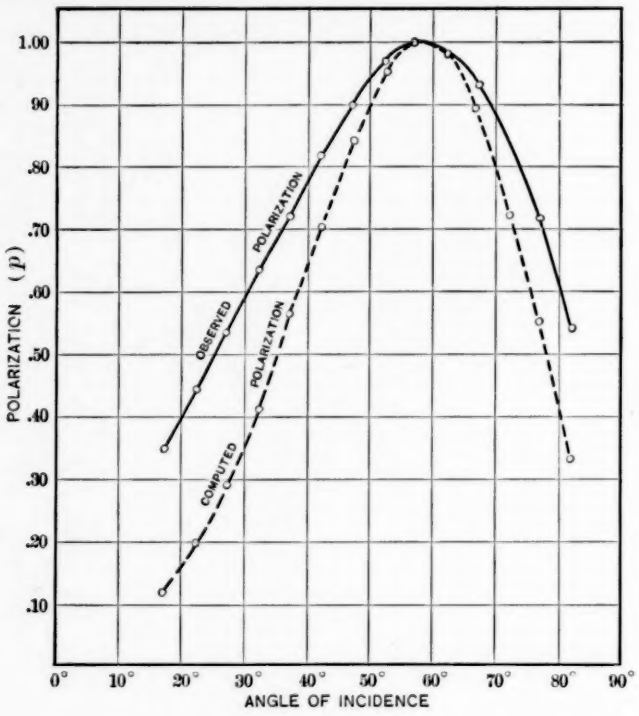


Fig. 9. Rubber.

the case of glass. The variation may possibly indicate a difference between the reflection from a transparent substance and that from an opaque substance, but the extent of the experiment was not sufficient to warrant a conclusion in regard to the difference.

PHYSICAL LABORATORY OF CORNELL UNIVERSITY.

On Latitude Variation in a Rigid Earth, as Illustrated by Maxwell's Dynamical Top.[1]

By Henry Crew.

THE remarkably large and accurate series of observations recently published by Professor George Davidson[2] has given additional interest to the question of latitude variation.

His paper raised the question, in my mind, as to the possibility of explaining this, now fairly well proven, phenomenon in terms of elementary dynamics.

The following affirmative answer is simply one which I looked up for my own satisfaction, and, though it contains no new contribution to knowledge, I have thought it worthy of presentation both on account of the renewed interest in this very old astronomical inquiry and also for the sake of calling your attention to this beautiful, but much neglected top, which Maxwell first spun at Edinburgh some forty years ago.

First of all we are not considering the geocentric or geodetic, but simply the astronomical latitude, viz., the angle between the plumb line and the celestial equator, or if you choose, the altitude of "the celestial pole."

For the pole, *as observed by upper and lower culminations*, is a direction which is fixed in space so far as the motion of the earth is concerned. It is what we shall later call the "invariable pole." The plumb line, on the contrary, is fixed in the earth, considered as a rigid body, and hence moves with it.

Having then one line fixed in space and another in the moving body, one can, by actually measuring the angle between them, detect any change in the axis about which the body is rotating; that is, one can detect whether or not the earth shifts about the axis of original spin with any motion other than that of daily rotation. For such a determination observations by Talcott's Method are appropriate, since they give the zenith distances (or what, for our purpose, is the same thing—*differences* of zenith distances) of stars of known declination. And declinations, it must be remembered, are referred entirely to an outside system, viz., the celestial equator and the fixed stars.

[1] Being a paper read before the Science Club of Northwestern University, 8th of February, 1895.

[2] Astronomical Jour., No. 323.

Having now acquired a perfectly definite understanding of what is *meant* by latitude variation, our next inquiry shall be what right have we, on dynamical grounds, to *expect* any such variation as that described?

Before taking up this question it will be necessary to state the

Conditions of the Problem.

When compared with the most general motion of a solid body, the question is a very simple one; for we shall assume, first, what Mr. Chandler and Professor Newcomb[1] have recently shown is not allowable, if we wish to predict the motion as completely as possible.

(1) *that there is no relative motion of the different parts of the earth.* That is, all questions of elasticity and viscosity are ruled out.

(2) Secondly, *that no forces act upon the body.* All accelerations are, therefore, ruled out; and velocities must be expressible in terms of the constants of the body, including among these the constants which determine the initial circumstances.

(3) The third assumption is that the *body is fixed at one point*, viz., *the center of mass.* Thus robbed of three degrees of freedom, it can only spin about an axis through the fixed point. And, in virtue of the assumption of no forces, this rotation must occur without precession or nutation.

Illustration of the Hoop.

In order that everyone may clearly grasp the phenomenon we are studying, I may recall the very apt illustration employed by Maxwell, and aftewards used by Thomson and Tait, viz., that the motion of the earth is practically that of a circular hoop rolling, *but not slipping*, on a stick of circular cross-section. I say "practically" because the earth is elliptical, and not quite circular, in sections parallel to the equator.

Now imagine the axis of the stick fixed in space. And let us, therefore, call it the "*invariable axis.*"

The line of contact of stick and hoop is the axis about which the hoop is really rotating at any given instant. It is, therefore, called the "*instantaneous axis.*" The path of this axis in space is evidently the circumference of the *stick*; but its path *in the body* is the section of the hoop.

As to the path of the invariable axis in the body, it is also a circle, viz., one whose center is the center of the hoop and whose radius is the distance between centers of hoop and stick.

Now to these two axes we must add still a third, viz., the axis of the hoop which passes through its center and is perpendicular to its plane. This is known as the "*axis of figure*," and here represents the axis of figure of the earth. This is the axis whose motion *in space* we are tracing.

[1] Monthly Notices, p. 336–341 (1892).

It is frequently called the "true axis," and is what is meant in general when the "axis" or "pole" of the earth is spoken of without further modification.

(Experiment with hoop shown.) Observe that whenever the hoop rotates about the stick, the line joining centers of stick and hoop changes direction *in the hoop, i. e.*, the plane containing the three axes just described shifts position in the rotating body. This amounts to saying that the axis of figure has changed its azimuth with reference to the invariable axis—that the axis of the earth no longer points to the same place among the fixed stars; or, still again, if you choose, that the plumb line has shifted with reference to the celestial equator; or, in general, the latitude of every point changes as the body rotates. All this, of course, provided the motion of the earth *is* that of the hoop rolling on the stick.

Rate of Rotation of Pole of Figure.

So far we have considered only the *path* of the invariable axis in the body, and not at all its rate of rotation. The illustration, however, is competent to give us a clear picture of this part of the solution also. It is evident that, during each rotation of the hoop, the instantaneous axis advances along the hoop by one circumference of the stick. The rate, therefore, at which the invariable axis proceeds through the body is *nearly* one circumference of the stick "per day;" and one complete revolution of the invariable axis about the axis of figure will occupy *exactly* as many "days" as there are radii of the stick in one internal radius of the hoop.

This number has been computed for the earth, considered as a rigid body, and found to be approximately 306 days. Such observations as those of Professor Davidson show that here the circle described by the pole of figure is *less than 100 feet* in diameter. Corresponding to this, the diameter of the stick upon which the earth may be considered as rolling, is but a few inches.

Mathematical Theory.

What now remains for us is to show from simple dynamical principles that this hoop *does* represent the motion of a freely rotating rigid solid, fixed as its center of mass. The method employed by Maxwell[1] makes it necessary to introduce only two physical considerations, viz.:

(1) *The constancy of angular momentum of the rotating body.*

It is important to remember that this law is true only for rigid bodies, as illustrated by the fact that a cat always lights upon its feet, however it be dropped.

[1] Scientific Papers, Vol. I., pp. 248–262.

(2) *The constancy of kinetic energy of the rotating body.*

But these two experimental facts will be useful only after they have been given algebraic expression, so that one can see their consequences.

Axes of Reference.

It is well known that in any rigid body there can be described an ellipsoid whose center shall coincide with the center of mass, and such that the moment of inertia of the body about any radius vector of the ellipsoid will be numerically equal to the inverse squares of this radius vector. In any rigid body, there will be therefore, three mutually perpendicular directions about which the moments of inertia will be respectively maximum, minimum, and intermediate. These are the so-called "principal axes." Imagine such an ellipsoid described about the center of the earth, or, if you please, about the center of mass of this top.

In what follows, we shall use these three axes as axes of reference. Under no circumstances will any other be used; so that all angles and distances will be unambiguous. Call them x, y, z. Since these axes move in space, as the body moves, they are called "moving axes;" but for an inhabitant of the body they are fixed.

Definitions.

Next we shall need a few definitions. They are tedious, I know, but conversation without them is very difficult.

Let A, B, C = Moments of Inertia about x, y, z, respectively, taken in order of diminishing magnitude.

$\omega_1, \omega_2, \omega_3$, = Angular velocities about x, y, z, respectively.

l, m, n, = Direction cosines of axis of original impulse, *i.e.*, of the direction of angular momentum.

Then $$A^2\omega_1^2 + B^2\omega_2^2 + C^2\omega_3^2 = H^2 = \text{constant} \qquad (1)$$

expresses the first of the two dynamical principles assumed above; and

$$\left.\begin{aligned} l &= \frac{A\omega_1}{H} \\ m &= \frac{B\omega_2}{H} \\ n &= \frac{C\omega_3}{H} \end{aligned}\right\} \text{ define the } \textit{invariable axis}.$$

For the kinetic energy, we have

$$\tfrac{1}{2}(A\omega_1^2 + B\omega_2^2 + C\omega_3^2) = \frac{V}{2} = \text{constant} \qquad (2)$$

which expresses the other of the two physical considerations involved.

Here A, B, C are constants which are given so soon as the body is determined upon; but l, m, n, though defining the invariable axis, are themselves variable, being referred to moving axes; so also is the case with ω_1, ω_2, ω_3.

While we are not here especially interested in the instantaneous axis, it may make matters clearer to remark parenthetically that its position in the body is defined by the direction cosines p, q, r, where

$$\left.\begin{aligned} p &= \frac{\omega_1}{\sqrt{\omega_1^2+\omega_2^2+\omega_3^2}} \\ q &= \frac{\omega_2}{\sqrt{\omega_1^2+\omega_2^2+\omega_3^2}} \\ r &= \frac{\omega_3}{\sqrt{\omega_1^2+\omega_2^2+\omega_3^2}} \end{aligned}\right\} \text{Instantaneous axis.}$$

In accordance with our convention above, we now choose for the axis of x the *axis of figure* in the case of the earth, and then proceed to find the

Path of the Invariable Axis in the Body.

Here, pursuing the ordinary method, the time coördinates must be eliminated in order to obtain a general relation between the space coördinates. Choosing for this purpose Eq. (2) and eliminating the angular velocities by means of the defining equations for l, m, n, we have

$$\frac{l^2}{A}+\frac{m^2}{B}+\frac{n^2}{C}=\frac{V}{H^2} \qquad (3)$$

Let us, for a moment place

$$\frac{1}{A}=a^2$$

$$\frac{1}{B}=b^2$$

$$\frac{1}{C}=c^2$$

$$\frac{V}{H^2}=e^2$$

Then Eq. (3) becomes

$$a^2l^2+b^2m^2+c^2n^2=e^2$$

But if x, y, z, be any point on the invariable axis, at distance r from the origin, then

$$l = \frac{x}{r}$$

$$m = \frac{y}{r}$$

$$n = \frac{z}{r}$$

and equation (3) becomes again

$$a^2x^2 + b^2y^2 + c^2z^2 = e^2r^2$$

or

$$(a^2 - e^2)\,x + (b^2 - e^2)\,y^2 + (c^2 - e^2)\,z^2 = 0$$

which you see is the equation of a cone having its apex at the origin. The physical meaning of this is that the invariable axis, always passing through the fixed point, describes a cone in the body; or, in other words, the axis of figure describes a cone in space.

Knowing now the path of the invariable axis, it is a matter of the utmost ease to determine the path of the invariable pole in any plane we may choose, say the *yz* plane—a plane tangent at the north pole, if you like. The equation to such a plane is

$$x = k = \text{constant.}$$

Under this condition, Eq. (3) becomes

$$(b^2 - e^2)\,y^2 + (c^2 - e^2)z^2 = (e^2 - a^2)\,k^2 = \text{constant.}$$

And the same *form* of equation is obtained for any other plane. Just what curve this equation represents depends upon the relation between the moments of inertia $\frac{1}{b^2}$, $\frac{1}{c^2}$ and the quantity $\frac{1}{e^2}$

It must not be forgotten that e ranges in value only between a and c, since

$$a^2l^2 + b^2m^2 + c^2n^2 = e^2$$

where

$$l^2 + m^2 + n^2 = 1.$$

If $b > e$, the path of the invariable *pole* is evidently an ellipse. The physical meaning of this is that if, at the beginning of the motion, the invariable pole does not quite coincide with the pole of figure, it will never coincide. On the other hand, these two poles will never part company *very widely* except when b approaches very near the limit of equality with e, under which condition ellipticity of the path becomes very great.

If, in addition to the condition $b > e$, we have also $c = b$, the path becomes a circle. These are practically the conditions fulfilled by the earth, which is very nearly symmetrical with reference to its maximum principal axis, the so-called axis of figure. What one might expect, therefore, in the case of the earth, is to find a variation of latitude corresponding to this circular motion of the true pole about the pole of the heavens—provided

of course, the earth were not originally set spinning exactly about its axis of figure. And recent observations show almost, if not quite, conclusively that it was not.

There is another special case which happily is not realized in the case of the earth, but is easily shown with this top. Imagine the body set spinning about the y axis, the mean principal axis.

The path of the invariable pole in the plane $y = k$, a constant, will be

$$(a^2 - e^2)\, x^2 + (c^2 - e^2)\, z^2 = (e^2 - b^2)\, k^2.$$

But this is the equation of a hyperbola since always $a < e < c$. Since the hyperbola is a curve with infinite branches, it is evident that, if a rigid body be spun about its intermediate axis of inertia, the invariable pole *will start off toward infinity*, so to speak, along one of these branches. In other words, the motion becomes unstable. The physical fact, however, that the actual path of the pole must be a closed curve constitutes an interesting experimental demonstration of the theorem that the spherical projection of these hyperbolas are closed curves. The physical significance of these closed curves is that the invariable pole always follows one of them *away* from the mean axis and *about* a new axis—an axis about which it *can* rotate in stable equilibrium.

This dynamic instability is to be carefully distinguished from static instability such as results from displacing the center of support underneath the center of gravity.

Indeed, one can easily imagine a set of highly improbable circumstances under which the water of our planet might be diverted into polar ice caps to such an extent as to produce this very dynamic instability.

Period of Rotation of Invariable Axis about Axis of Figure.

We have now seen that the true pole once displaced from the invariable pole remains displaced and moves, in the case of the earth, in an ellipse about it. It remains to sketch hastily the method by which the angular velocity ("mean motion") of the invariable axis in the body may be determined. Consider any point, P, on the invariable axis at unit distance from the origin. Its coördinates will be l, m, n. Let V_x, V_y, V_z, be the linear speed of this point along x, y, z, respectively. Its angular velocity, say ω_x, about the axis of x then is

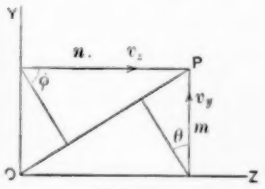


Fig. 1.

$$\omega_x = v_y \frac{\cos\theta}{\sqrt{m^2 + n^2}} - v_z \frac{\cos\varphi}{\sqrt{m^2 + n^2}}$$

$$= v_y \frac{n}{m^2 + n^2} - v_z \frac{m}{m^2 + n^2}$$

where θ and φ have meanings indicated in figure.

But
$$v_x = n\omega_2 - m\omega_3$$
$$v_y = l\omega_3 - n\omega_1$$
$$v_z = m\omega_1 - l\omega_2$$

Hence
$$\omega_x = \frac{ln\omega_3 - n^2\omega_1 - m^2\omega_1 + lm\omega_2}{m^2 + n^2}$$
$$= -\frac{\omega_1}{1 - l^2} + \frac{l(l\omega_1 + m\omega_2 + n\omega_2)}{1 - l^2}$$

or substituting[1] for ω_1, ω_2, ω_3, in terms of l, m, n, we have

$$\omega_x = -Hl\frac{a^2 - e^2}{1 - l^2}$$
$$= \frac{\omega_1}{1 - l^2}\left(\frac{e^2 - a^2}{a^2}\right)$$

This solution is complete from a mathematical standpoint; but the physical meaning is clearer when ω_x is expressed in terms of days. What we want to know is this: How many rotations does the earth perform while the invariable axis describes a complete cone, or, in other words, what is the period of latitude variation in term of days? This can be answered as soon as we know the area of the complete ellipse which the invariable pole describes and the area which it sweeps out during one revolution of the body. The ellipse, the base of our original cone, is

$$\frac{b^2 - e^2}{e^2 - a^2}y^2 + \frac{c^2 - e^2}{e^2 - a^2}z^2 = 1$$

Accordingly its area is

$$\pi\sqrt{\frac{e^2 - a^2}{b^2 - e^2}}\sqrt{\frac{e^2 - a^2}{c^2 - e_2}}$$

To determine the remaining factor, let θ be the angle which the invariable axis makes with the axis of x; and let r be the perpendicular let fall from the extremity of the invariable axis (of unit length) upon the axis of x. If ω_r be the angular velocity of the radius vector r about x,

$$\omega_r = \sin\theta \cdot \omega_x$$
$$= \sqrt{1 - l^2} \cdot \omega_x$$

The area described per second by r is

[1] It may be well here to recall the distinction between ω_1, and ω_x. The former is the x component of the total angular velocity, *i. e.*, the angular velocity about the instantaneous axis, while the latter is the x component about the invariable axis.

$$= \tfrac{1}{2} r\omega_r$$
$$= \tfrac{1}{2} (1 - l^2)\, \omega_z$$
$$= \tfrac{1}{2} \omega_1 \left(\frac{c^2 - a^2}{a^2} \right)$$

But the body rotates once in $\frac{2\pi}{\omega_1}$ seconds, whence the area described during one rotation of the body is

$$\pi \frac{c^2 - a^2}{a^2}$$

The ratio of these two areas is the quantity we want, viz., the period of the latitude variation. Let us call it T, then

$$T = \frac{\pi \sqrt{\frac{c^2 - a^2}{b^2 - c^2}} \sqrt{\frac{c^2 - a^2}{c^2 - c^2}}}{\pi \frac{c^2 - a^2}{a^2}} \text{ days.}$$

$$= \frac{a^2}{\sqrt{b^2 - c^2}\, \sqrt{c^2 - c^2}} \text{ days.}$$

$$= \frac{a^2}{\sqrt{b^2 - a^2}\, \sqrt{c^2 - a^2}} \text{ very approximately.}$$

The degree of approximation here introduced is quite allowable, since it only involves the assumption that l, the cosine of maximum latitude variation, is unity.

If we assume, what in the case of the earth is also very nearly true, that $b = c$, then

$$T = \frac{a^2}{b^2 - a^2} = \frac{B}{A - B}$$

the value of which is approximately 306 days, the Eulerian period. (Latitude variation, dynamical instability, and other phenomena shown with the top.)

It is a matter of some surprise that this beautiful instrument has not been more widely used in this country; for it has proven itself an invaluable aid in presenting the whole subject of dynamics of rotation. This particular top, due to both the skill and the kindness of our fellowtownsman, Mr. A. T. Merriman, runs from 10 to 15 minutes after having been spun at a moderate speed.

Maxwell's directions for making the top are so explicit that one cannot go astray in following them.

The phenomena of nutation and precession are shown with this quite as well as with the ordinary gyroscope; and in addition it exhibits num-

erous other phenomena such, for instance, as this. Adjusting the top so that it spins stably when carrying a paper color disc; the paper disc is replaced by one of sheet metal, just heavy enough to make the axis of spin the intermediate axis of inertia. You see that instability results; and we have an illustration of the effects of the possible, if highly improbable, heaping up of ice about the two poles of the earth.

Note added February, 1898.

Adjustment of Maxwell's Top.

I. After oiling bearing, set the six horizontal screws (See Plate) so that they each lack about $\frac{1}{8}''$ of being screwed in clear up to the head.

II. Set the bob (large nut) about $1''$ below the top *thread* of the steel axle. Clamp it with a check-nut.

III. Adjust the steel axle in the body of the top so that its lower end lies just a trifle (say, half a turn) above the center of gravity of the top. This adjustment is made, of course, by balancing the top on the supporting pillar. This adjustment made, clamp with check-nut.

IV. Take any two screws at opposite ends of a diameter, and unscrew them until they lack about $\frac{1}{2}''$ or $\frac{3}{4}''$ of being screwed in clear up to the head.

This will produce marked inequality among the three principal axes of the inertia ellipsoid.

V. Put the color disc on the top. It is now ready to spin *slowly*, unless possibly the color disc has made the top statically unstable, in which case the bell of the top must be lowered.

In beginning, it is wise always to use the spinning fork, never to spin the top very fast and always to be ready to catch the top with the hand in case dynamical instability occurs.

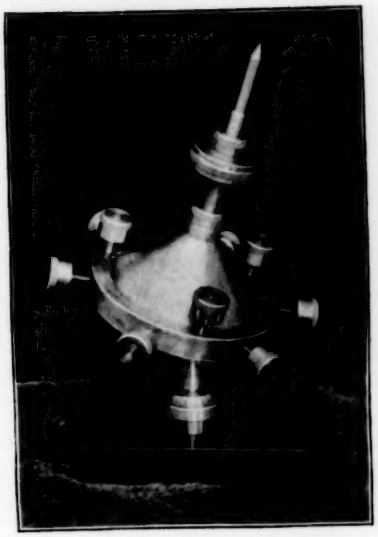
VI. *Adjustment of axle to coincide with axis of figure.*

When the top is first spun, as above described, it will, in general show a certain amount of "wobble." This is due to the mass not being symmetrically distributed about the axle.

To remedy this, observe the color in the disc into which the instantaneous axis runs. Then, if the steel axle be the axis about which the moment of inertia is a *maximum*, one must screw *down* that one of the three vertical screws which lies on the same side of the top as the color into which the instantaneous axis makes its excursion.

But if the steel axis, the axis of *least* moment of inertia, then one must screw *up* (*i. e.*, unscrew) this same screw. One easily discovers by trial which is the proper adjustment to make. Continue this adjustment until, on spinning, the top shows no "wobble." The top is now in adjustment.

In demonstrating the top to a large audience, the disc should be il-



UNIV. OF MICH.

luminated by a strong light from above, the eyes of the audience being shielded from its direct rays.

The following phenomena are easily shown:

I. *Precession*—clockwise by screwing bob in one way. Counterclock by screwing bob in other way.

II. *Statical stability conferred by rotation.*

The top is set so as to be in evident static instability: on rotation it becomes stable.

III. *Dynamical Instability conferred by rotation.*

The top is adjusted to statical stability by lowering the bell. On setting the "bob" up or down a little, one soon finds a position where the axle is the *intermediate* axis of inertia. This position is at once recognized by the fact that the top now refuses to rotate about the axis of original spin.

The top is now stable only when at rest; and great care must be used not to allow the top to wreck itself.

A very simple way to make the axle the axis of maximum, intermediate, and minimum moment of inertia successively is as follows: Entirely remove the check out of the bob. Screw the bob down near the bell of the top. The top now spins stably; and, if it has been spun in the right direction, one can, by simply holding the bob between thumb and forefinger, bring it to any desired position on the axle, *without stopping the top*. As the bob moves up, the moment of inertia about the axle becomes *relatively* less and less, until it has passed from maximum to minimum, through the intermediate value.

IV. *Variation of Latitude.*

Spin the top slowly. Tap the steel axis smartly with a lead pencil. The instantaneous axis immediately leaves the center of the color disc and travels around the disc in an ellipse.

By screwing the "bob" into its *other* position of stability, the instantaneous axis travels around the disc in the opposite direction.

V. *Effect of Polar Icecap.*

Remove the color disc. Adjust the top so that it spins stably, but is *on the verge of dynamic instability*. Set the top spinning, and while it is still spinning, gently slip on the color disc, having in the meantime imagined sufficient brass taken from the equatorial region of the top to make up the actual color disc used.

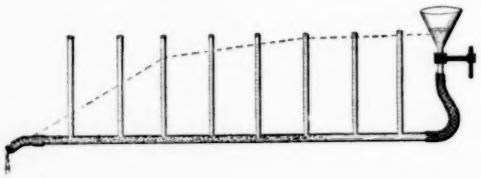
If the top has been properly adjusted dynamic instability will result, *i. e.*, the top will try to rotate about an axis lying in the equatorial plane.

VI. In addition to the above, the top is useful in illustrating other phenomena connected with rotational dynamics—especially in representing the parallel treatment of translational and rotational dynamics.

APPARATUS FOR ILLUSTRATING POTENTIAL GRADIENT.

BY R. W. WOOD.

THE potential gradient along a conductor of varying resistance can be well shown by the following piece of apparatus, which is so simple that it has very likely been used before, but which I have not seen described. A glass tube of 1 cm. bore, about 1.5 meters long, is provided with 8 lateral tubes, each about 40 cms. long and 4 mm. in diameter. A small roll of wire gauze is pushed into the tube until it is midway between the 2d and 3d side tubes, close to the latter. The tube is now filled with coarse shot (about No. 1) nearly up to the 6th tube and the rest of the way with fine shot (No. 8), a plug of gauze being inserted to prevent their exit. The gauze rolls must be very open, so as to offer little resistance to the flow. The empty end of the tube is connected by a rubber hose with a funnel or other reservoir, and a short piece of hose is slipped over the other end. The funnel is filled with water, colored with some aniline dye, care being taken that there are no air bubbles in the rubber hose, and the liquid allowed to flow through the tube. The fall of pressure along the tube is shown by the height of the fluid in the vertical side tubes. In the first two, where the resistance is small, the level is the same. In the next three there is a small uniform fall where the resistance is greater, and in the last section the gradient



is very steep, as shown in the figure. On closing the short piece of hose where the water flows out, the potential rises to a uniform height all along the tube. In filling the tube it is well to tip the outlet end up a little, and allow the water to run in very slowly to avoid enclosing air bubbles. If air bubbles get into the vertical tubes they can be removed by means of a small glass straw, made by drawing out a piece of fair-sized tubing until it is small enough to slip easily into the tubes. The apparatus can be set up on the lecture table with the rubber tube at the end closed, and is then ready for instant use. Strips of white paper should be put behind the vertical tubes.

PHYSICAL LABORATORY, UNIVERSITY OF WISCONSIN.

Apparatus for Showing the Conductivity of Gases.

By R. W. Wood.

THOUGH there is no new principle in the apparatus described in this paper, I believe that its compactness and permanency, and the fact that it is always ready for use without preparation, are sufficient grounds for presenting it to the readers of the Review.

It will be recognized as a modification of apparatus devised by Kundt for showing the conductivity of gases, and can be made by any one having some skill in glass blowing.

It consists of three bulbs, each with a smaller bulb in its center. The jet tubes communicating with the inner bulbs are drawn out until the bore is reduced to 0.5 mm. Reference to Fig. 1 will make the construction clear. Two of the outer bulbs are filled with gas—(carbonic acid and hydrogen respectively) and hermetically sealed, while the third is exhausted to the highest possible vacuum on a mercurial pump.

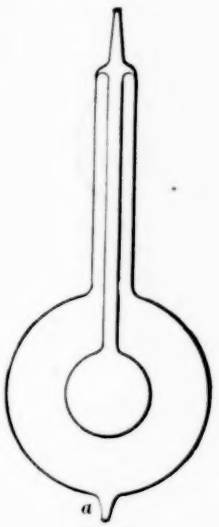


Fig 1.

The bulbs are mounted on a wooden rod with brass strips, and a little ether introduced into the inner bulbs by means of a pipette drawn out into a very fine capillary. On immersing the apparatus in scalding hot water, until the large bulbs are covered, the ether is vaporized at rates depending on the rate at which heat is fed across the space intervening be tween the outer shell and the inner bulb. In about half a minute the jet of ether vapor from the hydrogen bulb, can be lighted burning with a flame about 4 cms. in height. The second jet lights about fifteen seconds later, and burns with a much smaller flame, while the jet from the exhausted bulb does not ignite till the lapse of another half minute, and the flame is so small that it is almost invisible (Fig. 2).

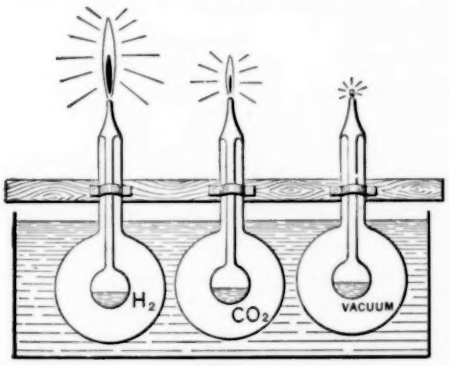


Fig 2.

A few words regarding the construction of the double bulb may be helpful. The outer

bulb is blown out of a piece of fair sized tubing first drawn out as shown in Fig. 5 and attached to a piece of smaller tubing about 10 cm. long and 8 mm. internal diameter; a piece of smaller tubing is now melted together at one end into a small drop, introduced quickly into the larger bulb, and blown out to the desired size. The small tube is now cut off so that about 4 mm. project when the little bulb is in the center of the large one. The inner bulb is now pushed against the wall of the outer, by means of a splinter introduced through the small neck, and the projecting end flared into a collar equal in size to the internal diameter of the larger tube by means of a carbon cone (Fig. 3). This collar is now pushed into or against the opening of the larger tube and a piece of tubing joined on as shown in Fig. 4. It is necessary to blow into the large bulb as well as into the smaller, as any one familiar with this sort of a joint will understand, and this is best accomplished by means of three pieces of small rubber hose and a T joint as shown in Fig. 4. After the joint is made the large tube adjoining it is drawn out at *e* into the jet. Careful annealing is necessary. It is not a very difficult job, but considerable skill is necessary to make the joints neat, and get the inner bulbs of uniform size and thickness, which is necessary to insure the proper working of the apparatus. The

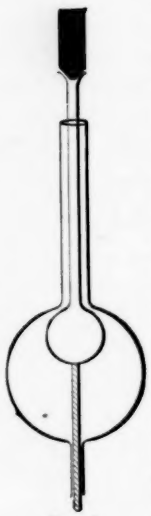


Fig 3.

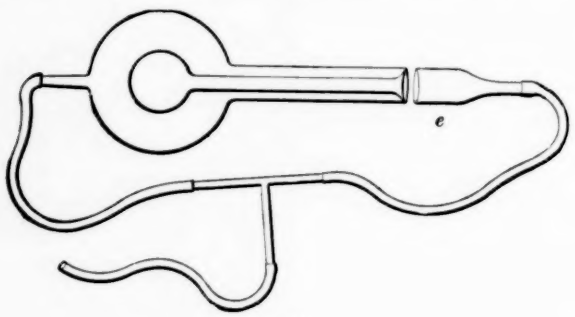


Fig 4.

Fig 5.

outer space is filled with gas or exhausted through the narrow neck (*a*. Fig. 1) which is then closed in the flame completing the apparatus.

PHYSICAL LABORATORY, UNIVERSITY OF WISCONSIN.

A Vacuum Tube to Illustrate the Slow Diffusion of the Residual Gases in High Vacua.

By Ernest Merritt.

In operating a Geissler pump it is often surprising to note how long it is necessary to wait between strokes in order that the pressure may be equalized in different parts of the pump. This is especially noticeable at high exhaustions, and whenever there is at any point a considerable contraction in one of the connecting tubes the slow diffusion often causes considerable delay. Crookes has called attention to this phenomenon in his papers upon high vacua, and, I believe, cites instances where it was necessary to wait an hour or more before measurements of pressure with the McLeod gauge could be made with consistent results. While lecturing recently on subjects connected with high vacua it occurred to me that it would be desirable to give an illustration of this phenomenon. The vacuum tube, or rather the pair of tubes, which is described below, was therefore constructed for the purpose.

Two ordinary cylindrical tubes with aluminum terminals at the ends were connected by a capillary tube about 20 cm. long, the latter being sealed into the walls in such a way as to make the combined tube present the appearance of a letter H. The bore of the capillary tube was about .5 mm. in diameter. Each of the two tubes connected by it was about 15 mm. in diameter and 20 cm. long. Connection to the pump was made through a short glass tube leading from near the end of one of the pair of tubes forming the vertical legs of the H.

During the exhaustion of this combination tube, air from the tube that is directly connected to the pump can readily flow into the latter at each stroke; but the exhaustion of the more distant tube can only take place as the result of the transference of air through the long capillary. At high exhaustions this transfer takes place very gradually, and the apparatus affords an illustration of the phenomenon described by Crookes upon a considerably exaggerated scale.

The condition of the vacuum in each of the two tubes is indicated by the character of the discharge sent through it by an induction coil. When the pressure has been reduced to about 3 or 4 mm. a characteristic Geissler discharge is shown in both tubes and no marked difference can be seen between the two; but as the exhaustion proceeds beyond this point, especially if the pump is operated rather rapidly, the vacuum in the tube nearer the pump is seen to increase much more quickly than that in the more distant one. A condition of affairs may readily be reached such that one tube shows all the characteristics of a rather high Crookes' discharge, the walls, for example, becoming brilliantly fluorescent under

the influence of the cathode rays, while the other tube still shows a stratified Geissler discharge, with no indication of cathode rays. This extreme difference may be maintained for a considerable time, and the gradual equalization of pressures in the two tubes is readily followed by watching the corresponding changes in the appearance of the discharge. After the pressure has been equalized by long standing, so that cathode rays are being developed in both tubes, the phenomenon can be shown in the inverse order by very gradually allowing air to enter the pump. It is important, however, that the air should not be permitted to enter too rapidly, for at pressures of a centimeter or more the air seems to pass through the capillary tube quite readily. It is only at high exhaustions that the phenomenon is shown in its most striking form.

Since the combination tube has four terminals it is possible to change the connections so as to modify the experiment in a great variety of ways. One of the terminals of one tube, for example, may be used as a cathode, while one of the terminals in the second tube forms the anode. The capillary connecting tube will then be brilliantly lighted, and often shows a distinctly different color at one end from what it does at the other. The cause of this variation in color I have not yet found time to investigate. It is quite possible that it is due merely to a difference in the density of the air; but a hurried examination of the spectrum indicates that there is a complete change of the more prominent lines in going from one end of the capillary to the other. In preparing the tube and filling it with air no precautions whatever were taken to avoid the presence of impurities. It is, therefore, quite probable that vapors from the flame, and possibly other impurities, were present. These would probably pass through the capillary at different speeds, so that it seems not improbable that by the slow diffusion through the capillary a nearly complete separation of the residual gases might have been produced.

When one terminal is used from one of the two tubes and the other terminal from the other, it is possible to have the conditions such that the region near the anode is stratified, while the region near the cathode shows the characteristics of a high vacuum Crookes' discharge. Upon reversing the direction of the current, the change in the appearance is most striking. In the first case cathode rays are developed in large amounts, in the other case there is no trace of these rays. This form of the experiment may be shown fairly well by one of the older forms of spectrum tube with the capillary somewhat smaller than that usually employed.

When the negative terminal is in the denser gas and the positive terminal in the rarer no cathode rays are developed at the cathode itself. But at the positive end of the capillary a sharply defined bundle of

cathode rays proceeds from the opening of the capillary straight across to the opposite wall of the tube. It is usually possible to see the luminous path of the rays through the gas as well as the fluorescent spot which they develop on the glass. The rays are readily deflected by a magnet and behave in every way as though the opening of the capillary were a real cathode. In fact, this form of the experiment affords as good an illustration of Goldstein's secondary cathode rays[1] as I have seen.

Another modification of the experiment with the H-tube is to connect the two in parallel. The discharge will then pass through the tube of least resistance, and the change in resistance due to pressure changes can be illustrated. This arrangement is, however, not so satisfactory as to connect the two cathodes only in multiple, while using a single anode. The discharge will then divide between the two cathodes. If the experiment is performed before the pressures in the two tubes have been equalized by diffusion through the capillary, one of the cathodes shows the blue glow characteristic of low vacua at the same time that the other is giving off an abundance of cathode rays. Upon reversing the current, so that the two *anodes* are in multiple, the discharge passes from one anode only, namely, from the one nearer the cathode.

It will be seen that the *H*-tube described may be used for a great variety of purposes in the way of illustration. It has the disadvantage that it must be kept in connection with the pump; for, although the difference in pressure between the two tubes will last for a long time, the two ends of the capillary will ultimately reach the same pressure. This difficulty might be avoided by using a stop cock to shut off the connection between the two tubes. Probably a tube of caustic potash on one side would serve the same purpose. There are, however, so many advantages in carrying on the exhaustion in the presence of the class, that it seems to me desirable to do so in experiments of this kind whenever it is possible.

[1] Goldstein, Wiedemann's Annalen II, p. 832, 1880.

NEW BOOKS.

Traité Élémentaire de Méchanique Chimique fondée sur la Thermodynamique. P. DUHEM. Two volumes. Paris, 1897.

This excellent book gives a systematic treatment of the applications of thermodynamics to chemistry at a time when current memoirs are already widely departed from the elements of the subject and the need of such systematic treatment is seriously felt by students of physical chemistry. It is particularly gratifying that one so preëminent among specialists in "chemical mechanics" should undertake the work, giving us a treatise which may "fairly be regarded as the best extant exposition of the subject which it treats."

The first volume is devoted to the general theory of thermodynamics and to the theory of what Duhem calls false equilibria. The second volume is devoted to vaporization and analogous phenomena and to dissociation.

The foundations of mathematical theory should always be those things which are sensible and measureable, never the "supposed smoothness of molecules or other rubbish of this fancy sort." Duhem's treatise meets the most exacting requirements in this respect.

Applied algebra, however, by no means embraces the whole range of physical theory; indeed concrete theories, such as the molecular theory, are much more important to the beginner than the purely algebraic theory. The beginner must master ideas, images of things, sufficiently abstracted to meet in the mind. Mathematical theory and concrete theory are of course not to be compared, being supplementary to each other. The latter has mainly to do with discoveries and applications, while the former follows with refinements and elaborations.

The writer is of the opinion that for teaching purposes, even for advanced students, an abstracted algebraic theory is less effective than analysis[1] in which ideas play a dominant part. Maxwell's classical treatise on electricity and magnetism is perhaps the best example we have of the latter style. There is no doubt a certain mathematical incompleteness in such treatise, but it seems that the various mathematical treatises which have been written with a view to the elucidation of Maxwell should *follow* the study of Maxwell to clear up the vexatious algebraic

[1] The writer has questioned a number of the leading American teachers of physics and engineering and is surprised to find that the prevalent notion of "analytical mechanics" seems to be that it is the formal application to mechanics of algebra including calculus!

questions of limits, continuity, and invariance, for the writer's experience is that almost any student having an elementary knowledge of physics and mathematics can, with some help from a teacher, fully comprehend Maxwell's treatise. Surely it will not always be necessary for students to reëstablish the fundamental notions in physics by algebraic analysis when a few generations of experience transcends all algebraic analysis in its substantial verification of the notions which we apply to the physical world around us; and it seems that concrete theory—that help for the mind corresponding to tools for the hand—must continue to grow in its range and importance. At present, however, the manifest inadequacy of our notions of electricity and magnetism is such that the purely algebraic aspects of our theories must be fully examined by a student before he has mastered the present state of the science. The writer cannot refrain in this connection from a word in praise of Webster's recent treatise on the mathematical theory of electricity and magnetism.

Thermodynamics, however, does not involve such a wide variety of geometrical and physical notions as does the subject of electricity and magnetism, and the theory of thermodynamics is perhaps to remain purely algebraic. However this may be, the writer does not share the author's regrets in being "*décidé à renoncer à une exposition absolument rigoureuse des principes de la Thermodynamique.*

It seems to the writer that the application[1] of the summation $\Sigma \frac{dQ}{T}$ to an irreversible or sweeping process is erroneous for the reason that, during a sweep, a system has no temperature since temperature is a mark only of a state of equilibrium. The error of this application has indeed been pointed out by Gibbs. However, the summation $\Sigma \frac{dQ}{T}$ has a definite significance in connection with a sweeping process as follows: Consider a sweep A which brings a system from a state of equilibrium 1 to a state 2, and a reversible process B which brings the system from 2 to 1. The above summation may then be applied to this process B, which is in a sense the reverse of A, giving $\sum_B \frac{dQ}{T} < 0$. Indeed the summation $\Sigma \frac{dQ}{T}$ may find application during the process A in case an adjoining system undergoes a reversible process under the influence of a sweep A, namely:

$$\Sigma \frac{dQ}{T} - S_1 + S_2 = P^2 \qquad (1)$$

in which S_1 and S_2 are the values of the entropy[3] of the given system in

[1] Duhem, Vol. I., p. 81.

[2] Equation (11). Duhem, Vol. I., p. 82.

[3] That is $S_2 - S_1 = \sum_B \frac{dQ}{T}$ in which B is the process before mentioned.

its initial and final states 1 and 2 respectively, P is a positive quantity, and $\Sigma \frac{dQ}{T}$, applied to the external system which undergoes a reversible process under the influence of the sweep A, is the increase of entropy of that system. The results of Duhem's analysis are not affected by his ostensible application of the summation $\Sigma \frac{dQ}{T}$ to a sweeping process; the only effect of the error, if such it really is, is to confuse the reader and to give apparent support to some statements made by the author regarding thermodynamic potential.

Duhem's claim[1] that the use of the thermodynamic potential is less ambiguous than the use of the entropy (method of Horstmann) in the study of a given system in that it takes more explicit account of the action of external systems, seems to the writer to be unwarranted. This idea grows out of the application of the summation $\Sigma \frac{dQ}{T}$ to a system during a sweeping process.

A sweeping process is essentially independent of external action upon the system which undergoes the sweep and a system may undergo any given sweep without exchanging heat or work with external systems. This is evident when we consider that a sweep is the passage of a system from an unstable state to a state of stable equilibrium. An apparent exception is afforded by any system subject to external action which changes at a finite rate. In this case the system falls behind as it were. A real exception is the sweeping process which iron undergoes as it is magnetized, but it must be remembered that the magnetic state of iron cannot be specified by a finite number of generalized coördinates and is therefore not amenable to the principles of thermodynamics as we at present know them. Perhaps the process of crystallization is a real exception also. If so, then the state of a system consisting of crystals and a mother liquor cannot be completely specified by a finite number of generalized coördinates. The question in regard to crystallization is simply this: Can a crystal be made to grow at an infinitely slow rate positive or negative and if so are the successive aspects of the system during the slow growth of a crystal precisely similar to the successive aspect of the system during the slow dissolution of the crystal? The phenomenon of etching of crystals seems to indicate that this is not the case, but, of course, the condition necessary to the production of etch figures is that the dissolution of the crystal be as rapid as possible. It would be interesting to study the forms of crystals under the condition of slow positive and negative growth. Indeed, the writer has undertaken to carry out observations along this line.

[1] Vol. I., p. 88.

Systematized physical science treats of phenomena associated with states of equilibrium or with material conditions which may be characterized by a finite number of definite (corresponding) elements, generalized coördinates, [1] generalized forces, rates, etc., such as volumes, pressures, reaction velocities, and the like. When a given set of physical conditions is reduced to its elements the science of the thing is accomplished, tentatively if the elements are not fundamental. Every bit of systematic knowledge in the whole range of the physical sciences exemplifies this fact.

It seems that a complete method for the study of sweeping processes, in the widest sense of that term, cannot be built up from our present methods of thought at the bottom of which is the idea of correspondence, or of cause and effect, if one so chooses to call it; for in the general aspect of the sweeping process this principle of correspondence fails. The decrease in thermodynamic motivity (Lord Kelvin) during a sweep constitutes an activity the results of which may be made to diverge widely by some infinitesimal circumstance which occurs during the initial stages of the sweep. This is shown by any system which sweeps from a state of unstable equilibrium into a state of stable equilibrium. Many sweeps do not indeed take their rise in states of unstable equilibrium, for example the sweeping process which a substance undergoes while the external actions on the substance change at a finite rate; such sweeps are reducible to elements among which the rates of change of the external actions appear. The most general type of sweep is that which brings a system from a state of unstable equilibrium to a state of stable equilibrium. The changes of the atmosphere which constitute weather phenomena are the most familiar examples of the general type of sweep. A state of unstable equilibrium is produced by the heating of the lower strata of the atmosphere. An infinitesimal action, as the waving of a fan, may precipitate a sweep, and slight variations of the circumstances, time and place of the beginning of the sweep may produce modifications in the progress of the sweep which are absolutely incommensurate to these causes, or in other words, an infinitesimal cause may produce a finite effect.

Long range detailed weather prediction is therefore impossible, and the only detailed prediction which is possible is the inference of the ultimate trend and character of a storm from observations of its early stages; and the accuracy of this prediction is subject to the condition that the flight of a grasshopper in Montana may turn a storm aside from Philadelphia to New York!

[1] Energy—the kind we think about—is nothing more than a generalized notion not to be properly thought of as existing outside of one's head except for the sake of concreteness.

A sweeping process—and no others are realizable—is then more or less a thing of chance, if one may use this word to characterize the failure of the principle of correspondence. This same conclusion is reached by the molecular theory in the study of gases. Indeed, it seems that the atomistic method is the only method capable of being extended to the study of sweeping processes, for where the different parts of a substance are in different states a study of the substance requires explicit consideration of these parts, but of course this is in general impossible. In those cases in which the behavior of a substance may be represented by a finite number of elements the substance must be homogeneous, or composed of a number of homogeneous phases; and every one of these phases, if not in thermal equilibrium, must deviate therefrom in a manner which is precisely the same[1] throughout its mass. Examples of such simple sweeping processes are to be found in the various cases in which reactive velocities have been studied.

Whether a method can be established for studying a substance by infinitesimal parts by the use of infinite series, which by more or less extended summation give approximate results, cannot be predicted, although it seems that simple sweeps might be followed in this way. For example, it would not be difficult to formulate the behavior of a gas expanding in a cylinder against a piston moving at a given velocity.

It can be shown from the principles of thermodynamics, as we know them, that the condition of thermal equilibrium, namely, that the variation of entropy for any virtual change in the system is zero or negative, must always have a unique solution so that only one state of equilibrium is possible for given external actions on a system. Now the most striking case in which the fundamental assumption[2] of thermodynamics fails is the case of magnetized iron, and Ewing has shown that the facts in this case may be represented by assuming that the process of magnetization is a process of which the successive aspects are states of equilibrium, infinite in number, separated by intervening sweeps; and the writer is inclined to think that the extension of the principles of thermodynamics which will finally include magnetization, and what Duhem calls *false equilibria*, will be an extension which admits the possibility of more than one solution of the condition of equilibrium. Indeed, the writer is inclined to look upon the conceptions of viscosity and friction which Duhem makes use of in his treatment of false equilibria as being as fanciful as, for example, the assumed smoothness of molecules—except, and it is a very essential

[1] In general, the deviation, at a point, from homogeneity and from thermal equilibrium must be known functions of the point. It is only those cases in which this function is of the simplest form that have been reduced to their elements.

[2] That the state of a system in equilibrium may be specified by a finite number of generalized coördinates.

exception, that the conceptions of viscosity and friction apply to things large enough to be felt and seen. Duhem's treatment of false equilibria may, therefore, be a false treatment of true equilibria. This, however, no man knows, and the treatment which Duhem gives is extremely valuable, being, as it is, the first systematic treatment of these phenomena.

This treatise of Duhem's is about as satisfactory as it is possible for such a treatise to be in the present state of our knowledge of thermodynamics.

W. S. FRANKLIN.

PHYSICAL LABORATORY, SOUTH BETHLEHEM, January, 1898.

Elementary Text-Book on Physics. By WM. A. ANTHONY and CYRUS F. BRACKETT. Revised by WILLIAM FRANCIS MAGIE. 512 pp. John Wiley & Sons, New York, 1897.

The supreme test of merit in a book whose object is to set forth the principles of a science is to be found in its *teaching value*. If this is a truism—and it reads like one—it is not always apprehended. We have so many class-room texts and of such varying worth, so many cases of much meritorious matter hopelessly involved in the subtleties of its own expression, and (alas!) so many instances in which a professed desire to instruct has been over-ridden by unconscious pedantry, that a moment's consideration of the subject, at this time, may not be inappropos. If my remarks are construed as championing the cause of the student, I shall not complain. It is, perhaps, time that this worthy were spoken for.

As a piece of literature, the "text-book" must ever occupy a limited sphere. Essentially didactic and in no degree creative, it may possess only the sterner graces of composition. There must be careful diction and concise phraseology. There must be logical sequence and, as to substance, always, of course, strict adherence to scientific truth. But in order that a book may be in the highest degree *teachable*—if I may use the term objectively—it must possess certain more subtle and indefinable qualities as a part of its birthright. With the limitations of style referred to, the writing of a scientific text-book would seem a prosaic enough task—one furnishing few opportunities for the impress of the author's personality. And yet I venture to assert that two treatises on the same branch of science, the individual products of two men of fairly equal intellectual attainments, will differ markedly in what I have called *teaching value*. After all, the work is likely to be the child of the man. "Your born teacher is as rare as a poet, and as likely to die young," writes a member of the pedagogical calling. Quite as rare, and perhaps as ephemeral, is the class-book in which is combined unquestioned scientific merit and the fullest power of yielding up the matter between its

covers. It would be false, however, to ascribe the abundance of instruction books entirely to lack of the essence of teaching value. Many things determine the choice of a class-book. The time limit, the previous preparation of the students, the nature of their courses—whether general or technical—are prominent factors. Rarely does an instructor find just the book he wants. Not infrequently he is compelled to prepare notes as the only way of meeting the exigencies of his case. It often happens, too, that these notes, after going through the threshing process of class-room use, are published. So, presently a new "text-book" is born, a new title is added to the bookseller's list, and the reviewer (poor mortal!) is invited to the christening.

To still further digress, I would plead for greater diversity in the statement of scientific truths. An author feels, no doubt, that in presenting a principle in as many lights as possible he may be guilty of looseness of style and, perchance, of some inaccuracy of statement. But while entertaining these fears, he should yet remember that he is setting lessons, not for the individual, but for the class, and that in every class there are men who, at the outset, can see a thing in only one way—their own way, the way, in fine, which their previous training and mental habits make possible for them. It may be urged that it is the province of the instructor to interpret and amplify the text to the needs of his class. To a certain extent this is so, but, in mercy to the instructor, the student is expected to prepare his lesson before coming to class, and if he has spent two-thirds of the preparation period in a vain attempt to make sense of a ponderously worded paragraph or to read in it a great deal more than really exists, he is likely to come to his class without his lesson and in its place an aching void.

Possibly if the writers of class-room texts would strive continually to recall the days when they too sat on the benches, we should be served with more efficient educational literature.

As for physics, its laws and phenomena fortunately admit of varied statement, seeing that Nature exhibits them to us in protean forms. The chief value of the subject is by no means disciplinary. It seems unfair to the cause of the advancement of science to prepare a text-book in physics that is not clarified to the highest degree consistent with rigor. To this end let us have the side-lights turned on to the full. Sufficient unto physics are the inherent and unavoidable difficulties thereof.

Prior to its revision by Professor Magie, the work before me passed through no fewer than seven editions. Herein is strong testimony to its sterling merit. The book was, on its first appearance, a radical departure from recognized forms in works of a similar nature. It was intended to promote the lecture system of teaching physics. Former works for undergraduate study had been prepared to meet the needs of classes in in-

stitutions whose equipment of physical apparatus was meager. As an offset to this deficiency, it was necessary to incorporate in the instruction book lengthy descriptions of apparatus and methods. Principles were swamped, as it were, in a sea of detail. Meanwhile the volume took on an inconvenient bulk.

Breaking quite away from precedent, the authors of this book started out with the avowed intention of expunging all "pictorial representations of apparatus," and a glance through its pages is sufficient to show that the intention has been carried out. One cannot help feeling that the illustrations of the cathetometer, the dividing-engine, and the spherometer, which appear in the first few pages, are not in strict conformity with the purpose set forth in the preface. However, the picture, pure and simple, is suffered no further incursions. Gravitation is treated without the leaning tower of Pisa, and electrostatics is embellished with neither Franklin and his kite, nor that charming old woodcut of Hawksbee exhibiting VonGuericke's friction machine. In relegating these old friends to obscurity, much space is economized, although at the expense of a certain austerity of aspect not altogether prepossessing to the beginner.

And, indeed, the work is not easy, as many an unlucky collegian could testify. To characterize the treatment by a single adjective, it is *severe*. There is about the entire book, a Puritan inflexibility of statement, agreeable enough to the student endowed with what has been called "the physical instinct" (whatever that may be!), but harassing to the average undergraduate. And remembering that the book is "elementary," this would seem to be its greatest failing.

The style of the work is clear-cut and forcible, a little heavy here and there, but always of a dignity befitting the subject-matter. The book appeals strongly to the thoughtful student; and to him who afterwards gains some familiarity with the other texts, there is a real delight in turning back to these pages so pregnant with meaning—albeit the delight occasionally comes from the recognition that a certain paragraph is now clear for the first time.

The work of the reviser is most manifest in the portions of the work devoted to mechanics and electricity and magnetism. The revision is well done and the general tone of the authors' treatment admirably sustained. It can be no easy task to revise a work without producing any of the usual effects of "new wine in old bottles." At the outset, *mass* is referred to as quantity of matter, while later, in Art. 22, the reviser exhibits it as the proportionality constant in Newton's second law. No attempt is made to reconcile these definitions in the mind of the student. The definition of *force* as the assumed cause of an observed change in momentum, which appears in the original, has been replaced by a discus-

sion of the subjectivity of force. Pretty strong "wine" this, for the beginner.

In the portion of the book devoted to sound, there are few changes. Professor Magie has placed the subject of sound immediately after mechanics, instead of letting it follow electricity as it was done in the original. One also notes that the discussion of the velocity of sound appears under the head of "Origin and Transmission"—surely a more fitting place than that accorded it by the authors. While the chapters on sound are excellent, they have not been brought up to date as have many other parts of the book. Recent advances in the pure physics of sound do not seem to have any place.

To the very able treatment of heat found in the original, there has been added a discussion of the kinetic theory of gases.

In electricity and magnetism there is much that is new, as before stated, although the same number of chapters appears and with the same headings. The reviser acknowledges his indebtedness to J. J. Thomson's Elementary Theory of Electricity and Magnetism—a most admirable and contemporary basis of revision. The added matter in magnetism contains a good discussion of Ewing's extension of Weber's theory and of the phenomena of hysteresis.

What are usually called the practical applications of electricity are accorded but brief mention. This is in keeping with the spirit of the book. Still, it is, perhaps, unfair to the subject of alternating currents to dismiss it in a single paragraph a page in length. The expression for the maximum value of an alternating current,

$$\frac{e}{\left(r^2+\frac{4\pi^2L^2}{T^2}\right)^{\frac{1}{2}}},$$

is obtained under the assumption of an electromotive force, not only "periodic," as stated, but harmonic as well. Is it not better to employ "periodic" in the more general sense? No mention is made of the effect of capacity in causing a phase displacement.

It is pleasing to note that the nomenclature of the book is of the kind that has gained an established foothold—the kind that is old-fashioned without being out of fashion. With the advance of a science its terminology necessarily grows apace, but it is dubious expediency to encumber an elementary text with the latest products of enthusiastic word-coiners.

There is also an agreeable absence of such expressions as "it can be proved," "it can be shown," and the like. They are a bugbear. The book is free from typographical errors and well printed.

Professor Magie's revision of this work must surely take rank, as did the original, among the few standard treatises on elementary physics.

C. P. MATTHEWS.

The Calculus for Engineers. By JOHN PERRY. Second Edition. Pp. vi+378. London and New York, Edward Arnold. 1897.

This book consists of an interesting and miscellaneous collection of mechanical and electrical engineering problems. The three chapters are "The Study of x^n," "The Compound Interest Law and the Harmonic Function," and "General Differentiation and Integregation." As typical of the arrangement of the book, under the Compound Interest Law we find: *Example 1*, An electrical condenser discharging through a resistance; *Example 2*, Newton's law of cooling; *Example 3*, A problem concerning the tensile strength of a tapering rod; *Example 4*, Compound interest; *Example 5*, Slipping of a belt on a pulley, etc. The book is not so much a calculus as it is a work on sundry applications of mathematics. It is doubtful whether it could well be used instead of other works on the calculus, although it would prove a valuable supplement to them.

The style is familiar and chatty, the author commonly using the first personal pronoun, addressing the reader in the second person, and giving him kindly (we might say paternal) advice upon the matter in hand and other subjects which chance to suggest themselves.

An extensive and good treatment is given of alternating current problems solved by symbolic methods, problems which ordinarily are dealt with in treatises on alternating currents. The solutions on the whole are admirably direct. The author might be reminded that the "secohm" has passed into history, and that the "henry" is now used in its place. Apparently through inadvertence the author has made the unfortunate error of assigning to the henry one-tenth the value ascribed to it by the Chicago Electrical Congress in 1893.

Many problems are solved which are of value to the electrical engineer, but a knowledge of these problems aside from that gained by this book is needed to make many of them intelligible. Likewise some paragraphs on subjects pertaining to electrical engineering are given in which no calculus whatsoever appears; *e. g.*, the section "True Power Meter," page 209. This section may be criticized first as irrelevant, since it is not calculus; second, because not intelligible to a reader not already acquainted with the subject; (and thirdly, we suspect that the compensating coil does not eliminate error as claimed, inasmuch as EG and CD act inductively on each other). The first and second of these criticisms apply to many portions of the book; and yet there is much to be gained in a careful study of these problems.

After a discussion of indeterminate multipliers, the advice is given that "on the whole perhaps the engineer had better forget this method * *," advice which might well have been given at the beginning rather than at the conclusion of the discussion.

The book will be useful in giving facility in the practical application of the calculus.

FREDERICK BEDELL.

Electricity and Magnetism. By ERIC GERARD. Translated from the fourth French edition under the supervision of DR. LOUIS DUNCAN by R. C. DUNCAN. Pp. xii+392. New York, W. J. Johnston & Co., 1897.

The main portion of this book is the translation from a work which has taken a high position on the continent. In the translation, chapters referring to electrical machinery and to special subjects, such as batteries and transformers, have been omitted, inasmuch as they referred almost exclusively to European practice, and furthermore, because the matter contained is easily accessible in other works. The translation retains those parts of the work relating to the general subject of electricity, and these are clear and adequate. In addition, three chapters have been added by Messrs. Steinmetz, Hutchinson and Kennelly, respectively.

Chapter 3 on "Hysteresis and Molecular Magnetic Friction" by C. P. Steinmetz, contains an admirable review of the subject in about thirty pages. On this subject no one can speak with greater authority than the writer of this chapter. His writings on the law of hysteresis and kindred subjects have proved a mine for many compilers. Unfortunately the original papers are rather involved and information on the subject cannot be readily gained from them without much study. The work of abstracting the valuable material from them can be done by no one so well as the original writer, and this has been done by Mr. Steinmetz in the preparation of this chapter.

The book as a whole is well edited, as shown by its consistent notation and freedom from errors. As an exception we may quote on page 95 of the chapter by Mr. Steinmetz: "The area of the hysteresis loop, with the M. M. F. in ampere-turns as abscissæ, and with the magnetic flux in volt-lines ($=10^8$ lines, or one hundred mega-webers) as ordinates, is equal to the energy expended by hysteresis, in coulombs." We may question the terms "volt-lines," "lines" and "mega-webers." The use of the "coulomb" as a unit of energy is obviously a slip.

Chapter 6, on Units and Dimensions, by Carey T. Hutchinson, contains an able treatment in twenty-two pages, which is free from the dead-wood and antequated notions which commonly encumber such discussions. The recommendations of the congress of 1893 are given and employed. The weber, gauss, gilbert and oersted, adopted provisionally by the American Institute of Electrical Engineers as magnetic units are explained, and likewise the conflicting proposals for the weber and the gauss made by the British Association. The relation between these various proposals is clearly brought out. This chapter is a valuable one for reference.

Chapter 8, on impedance, by A. E. Kennelly, contains a nine page discussion of alternating currents, which is necessarily inadequate. It gives in brief the usual solution for circuits containing resistance, self induction and capacity when the electromotive force is harmonic and the various coefficients are constant. Impedance is treated as a complex quantity and impedances are combined in series or in parallels according to the method of complex quantities.

The style of the book is good and the matter is well set forth. The discussions are straightforward and to the point, and, while the book contains mathematical solutions where required, a pretentious use of mathematics is avoided.

FREDERICK BEDELL.

Transactions of the American Institute of Electrical Engineers. Volumes XII (1895) and XIII (1896).

If one were to attempt to measure the activity of the *Institute of Electrical Engineers* by the bulk of the annual volume, one would note a decided maximum in 1894 and a rapid falling off since that time. Certainly, whatever the cause may be, the latter of the two volumes under consideration is scarcely more than half as thick as that which contains the transactions for 1894. Upon inspection it appears that the difference is in considerable measure ascribable to the much more extensive programme of the annual meeting held in 1894. At this meeting more papers were read than are contained in the entire volume for the year 1896. In the latter year only five titles appeared upon the programme of the general meeting and one of these was in the nature of a committee report; while at Philadelphia, on the other hand, there were fifteen papers.

If the two latest volumes of the Transactions are small they are, however fully up to those of former years as regards the character of their contents. One notes a praiseworthy tendency towards the consideration of the scientific aspects of the various topics treated, together with the elimination of a good deal of material of only transient or secondary value. A new feature of the last year's work of the institute is that of topical discussions. The two which are reported at considerable length in Volume XIII, upon electric traction under steam railway conditions and upon the nature of the Röntgen rays, bring out very well the strong as well as the weak side of this method of carrying on the scientific work of the Institute.

The Institute, in spite of the recent hard times, the depressing influence of which has been especially felt among electrical engineers, shows a continued slow but steady growth in membership. At the close of 1894 there were 926 members enrolled; in 1895, 1,023; in 1896, 1,069.

With so large a number of members upon its lists and with the very considerable income derived from them, the scientific world has a right to expect from the Institute a large and increasing amount of useful work. The great problem is that of concentrating the efforts of its widely scattered membership. Various plans have been tried in the past with varying success. One of the most encouraging symptoms of an increased usefulness was the interest displayed in connection with the summer meeting of 1897. It is an experiment well worth repeating.

E. L. N.

Theory of Groups of a Finite Order. By W. BURNSIDE, M.A., F.R.S.; Professor of Mathematics at the Royal Naval College, Greenwich. Cambridge: At the University Press. 8vo., pp. xvi+388.

We learn from the preface that "the present treatise is intended to introduce to the reader the main outlines of the theory of groups of a finite order apart from any application," and that "considerable space is devoted to the substitution groups," because the author believes that, "in the present state of our knowledge, many results of the pure theory are arrived at most readily by dealing with properties of substitution groups."

As the book is written for the beginner the first chapter has been devoted to explaining the notation of substitutions. The following six chapters are devoted to the more important group properties, *i. e.*, to those properties which are independent of the concrete form in which a group may present itself. Then come three chapters on substitution groups in which we find a lucid exposition of the elementary parts of this subject. Two of the remaining five chapters are devoted to graphical representation and one to each of the following subjects: isomorphisms, the linear group and solvability.

This is the first treatise in which an author has confined himself to the theory of finite groups apart from any application. By restricting himself in this way the author has been able to present the subject in a more masterly manner, and to bring more of its parts up to date than would otherwise have been possible. Notwithstanding this restriction the task was a very arduous one and we may expect some inaccuracies.

The theorem that every group of finite order may be represented as a regular substitution group (p. 22) is called Dyck's theorem in the table of contents and in the index. This theorem was known long before Dyck's article appeared. In a more general form it has been proved in the much earlier work of Jordan, *Traité des substitutions*, p. 58, and it is explicitely stated on p. 60 of this work. It was also proved by Capelli in his article "Sopra l' isomorfismo dei gruppi di sostituzioni" *Giornale di Matematiche*, (1878), p. 45. On p. 189 an error which occurs

in the given Traité of Jordan is pointed out and corrected, while Jordan himself had corrected the same error more than twenty years earlier in the *Bulletin de la Société Mathematique de France*, vol. 1, p. 41. It is very unfortunate that our author did not give the beautiful theorems in regard to the limit of transitivity which are found in this article by Jordan, instead of the older and less practical one found on p. 152.

On p. 221 an Abelian group is made simply isomorphic to itself by writing the inverse of an operator after each of the operators of the group, and the statement is made that the group of order 2 is the only one in which each operator corresponds to itself in such an arrangement. It is not difficult to see that each operator corresponds to itself in every group which contains no operator whose order exceeds two. As we may make any operator of order 2, that is contained in such a group, correspond to any other one of this order and thus establish a simple isomorphism of the group to itself, the conclusion of the author that "it is possible for every group, except a group of order 2, to establish a correspondence between the operators of the group, which shall exhibit the group as simply isomorphic with itself" is correct even if he did not prove it completely. By simple isomorphism he clearly means a non-identical simple isomorphism, since an identical isomorphism is always possible.

Although the Italian mathematicians have contributed very much towards the development of the theory of groups, yet not a single reference to their work is found in the treatise under consideration. This is very unfortunate, especially since such copious references are made to the memoirs of several other countries and an erroneous impression in regard to the comparative mathematical activity in this direction is thus conveyed. While the work under consideration contains some inaccuracies in addition to those which have been pointed out, yet, on a whole, it is a very useful treatise and it seems better adapted "to introduce to the reader the main outlines of the theory of groups of a finite order" than any other work extant.

G. A. MILLER.

CORNELL UNIVERSITY, January 24, 1898.

Tafeln und Tabellen zur Darstellung der Ergebnisse spectroskopischer und spectrophotometrischer Beobachtungen. By TH. W. ENGELMANN. Leipzig, Wilhelm Engelmann, 1897.

The spectrum charts contained in this collection are of two kinds. One set gives a group of six colored spectra, arranged vertically one above another, with light lines indicating wave-lengths. These, in their coloring represent, sufficiently well for the purposes of the beginner, the appearance of the continuous spectrum. The location of the lettered Fraunhofer lines and likewise their wave-lengths are indicated. The use

of this chart consists in sketching in from direct observation the appearance of absorption spectra. The charts are drawn in the old-fashioned manner, namely, with red at the left hand. This gives diminishing wave-lengths as abscissæ as we pass from left to right. The distribution of wave-lengths, moreover, is that corresponding to an ordinary prismatic spectrum, such as would be obtained with a single prism of medium flint glass. This method of mapping spectra has the advantage of affording a picture corresponding in all respects to that seen in the eyepiece of an ordinary one prism spectroscope, but it would, in our opinion, be decidedly advantageous to accustom the student to plotting diagrams of the spectrum with equal spaces upon the base line corresponding to equal wave-lengths and with wave-length readings increasing towards the right hand. Such additional diagrams can, of course, be drawn in connection with the use of these maps. We have in the well-known work of Le Cocq de Boisbaudran a good example of the double method of plotting, to which reference has just been made. All the numerous spectra drawn in that work are shown both in prismatic and in normal aspects.

The other set of charts belonging to Professor Engelmann's series are intended for the graphical indication of spectrophotometric measurements upon absorption spectra. For this purpose a rectangular diagram is prepared corresponding, as to its base line, to the wave-lengths of the prismatic spectrum. The ordinates are divided into one hundred equal parts and serve for the plotting of an extinction coefficient for each wave-length of the spectrum. These charts will be found very useful in spectrophotometry, but here again it seems, in our opinion, wise to follow them by diagrams in which the spectrum is presented normally rather than in its prismatic form and in which wave-lengths increase towards the right. E. L. N.

Intermediate Course of Practical Physics. By ARTHUR SCHUSTER, Ph.D., F.R.S., and CHARLES H. LEES, D.Sc. pp. xvi + 248. London, Macmillan & Co., 1896.

Exercises in Physical Measurement. By LOUIS W. AUSTIN, Ph.D., and CHARLES B. THWING, Ph.D. pp. x + 198. Boston, Allyn & Bacon, 1896.

A Laboratory Notebook of Elementary Practical Physics. By L. R. WILBERFORCE, M.A., and T. C. FITZPATRICK, M.A. Part 1, Mechanics and Hydrostatics, 31 pp.; part 2, Heat and Optics, 51 pp.; part 3, Electricity and Magnetism, 39 pp. Cambridge, the University Press, 1896.

Physikalisches Praktikum. Third Edition. Revised and enlarged. By EILHARD WIEDEMAN and HERMANN EBERT. pp. xxv + 490. Braunschweig, Vieweg & Son, 1897.

Physics Course of the California School of Mechanical Arts. Part 1, Introductory Course of Physical Measurements. By GEORGE A. MERRILL and CAROLINE W. BALDWIN. pp. 85. Part 2, Electricity and Magnetism. By CAROLINE W. BALDWIN. pp. 76. San Francisco, Upton Brothers, 1897.

Physics for Grammar Schools. By CHARLES L. HARRINGTON, M.A. pp. 123. New York, The American Book Co., 1897.

Observation Blanks in Physics. By WILLIAM A. HAMMEL. pp. 42. New York, The American Book Co., 1897.

Physical Experiments, a Manual and Notebook. By ALFRED P. GAGE, Ph.D. pp. ix + 97. Boston, Ginn & Co., 1897.

A Text Book of Physics, largely Experimental. Third Edition. Revised and enlarged. By EDWIN R. HALL, Ph.D. and JOSEPH Y. BERGEN, A.M. pp. xvi + 596. New York, Henry Holt & Co., 1897.

While we possess in the English language as yet no single compendium or treatise upon physics which may be compared with the great German manuals of Wuellner or Winkelmann, or even with the French text-books of Jamin or Violle, the crop of small minor treatises, and particularly of laboratory manuals of an elementary character, is a large and increasing one. Of the nine laboratory manuals to which we wish to call attention here, four are intended for students of the grade corresponding to that of undergraduates in our American colleges, while the remainder belong to the lower schools.

Schuster and Lees, although their book is written for an intermediate grade of readers, do not hesitate in the general instructions, which precede the practical exercises, to go into many really elementary details. These instructions, however, deal with matters, the importance of which is such that it is never out of place to recall them even to more advanced students of experimental physics. The proper manner of subdividing a scale by eye estimates, the avoidance of errors of parallax in reading, etc., cannot be too often reiterated. The exercises outlined in this manual are of a useful and thoroughly practicable character. It is refreshing to find a British work on physics in which the metric system is adhered to consistently throughout.

The little laboratory manual of *Austin and Thwing*, which represents the first year's work of students in the Physical Laboratory of the University of Wisconsin, contains an interesting selection of quantitative experiments. Fully one-third of the book is devoted to tables and to general directions concerning the manipulation of glass and quartz, together with directions for soldering, cleaning mercury, the preparation of cements

and varnishes, and the construction of standard cells and of a water battery. This leaves 119 pages for the description of sixty-five experiments or exercises, and of this space a considerable portion is taken up in general statements. For these it would seem more advantageous to refer the student to a text-book. As it is, he will find it necessary to refer to some larger laboratory manual for the details of many of the experiments themselves. This is doubtless the intention of the authors, who make occasional references to the works of Kohlrausch and of Stewart and Gee. In nearly all laboratory manuals, the provision for work in sound is meagre; in Austin and Thwing's book for example it consists of three experiments. The collection as a whole, however, offers an unusual variety of material.

The laboratory notebook of *Wilberforce and Fitzpatrick*, in which is outlined the practical work laid out for medical students in the Cavendish laboratory, is simply a list of experiments to be used in connection with the text-books of Glazebrook, together with very brief indications of the apparatus to be used and of the method of manipulation. The directions for carrying out the experiments are very brief, occupying as a rule from three to ten lines. On the opposite page an outline is given, indicating in each case the way in which the results are to be worked up. The book, which appears in the form of three pamphlets, is without illustrations.

It is interesting to compare the work outlined for medical students in the University of Cambridge with that demanded of them in German universities, such as Erlangen and Kiel, where *Wiedemann and Ebert's* manual is used. In the third edition of the latter manual, which lies before us, the tendency towards the selection of methods which will be of especial interest to chemists and to students of medicine, is more manifest, even, than in the earlier editions. Wiedemann and Ebert, on the one hand, have almost completely suppressed the mechanics of solids excepting in so far as it deals with the principle of the balance and with a few other subjects of manipulative interest, and they have selected their materials with direct reference to the technical interests of their classes. The directors of the Cavendish Laboratory, on the other hand, have apparently adopted the view, for which much may be said, that medical students, while studying physics, should pursue that science without especial reference to its application to their professional work. The result of the tendency of Wiedemann and Ebert to specialization has been to produce a laboratory manual which, while incomplete in many respects, is especially full and satisfactory in certain portions of the subject. As regards methods of determining densities, in the study of the properties of gases and in certain portions of the domain of heat, the treatment is excellent. Under the head of spectrum analysis, also, and of the polarization of light, a great deal of valuable material not to be found elsewhere in any single work has been amassed.

The arrangement of the *Laboratory Manual of the Physics Course of the California School of Mechanical Arts* (*Merrill and Baldwin*) is closely allied to that of the laboratory notebook of Wilberforce and Fitzpatrick. It is designed, however, for a very different class of students, those namely of a manual training school. The work is altogether quantitative, consisting in Part I. almost entirely of measurements of length and mass. The instructions are all exceedingly brief, and the description of the apparatus to be used consists, in most cases, simply of a list of the articles needed. An abundance of space is set aside for the tabulation of observations, and there are numerous blank pages upon which the student is expected to write out the results of his work. Part II., which is the work of Miss Baldwin alone, contains much more of physics proper and a smaller proportion of mere practice in the measurement of the dimensions of bodies than does the first part.

The remaining books upon our list are written with especial reference to the needs of the primary and secondary schools. Of these *Harrington's Physics for Grammar Schools*, indeed, is stated to be the result of several years' use of the experimental method in teaching physics to boys under thirteen years of age. The little book contains many experiments which will interest boys, but there are, on the other hand, many which it would have been better to have omitted altogether; take, for example, experiment 2, which reads as follows: "*Try to put any two objects into the same space at the same time.*" Very great care needs to be taken in the selection of experiments from which beginners, especially when those beginners are children, are expected to draw conclusions. It is safe to say that a considerable proportion of the children who, in experiment 4, are told to "*place in the sun's rays a goblet of water,*" and are then told that *sunlight occupies space*, and are asked whether they think from their experiment that sunlight is matter, would draw precisely the opposite conclusion from that intended by the author of this book. There are too many experiments in this book like No. 12, in which the pupil is directed simply to "*move a book from place to place,*" and too few in which he is given an opportunity for making measurements.

Along with the type of experiments to which reference has just been made, and which might well have been omitted from the book, Mr. Harrington has brought together a large number of others which will set children thinking and which will be of true service to them.

Hammel's Observation Blanks in Physics are likewise intended for beginners. They are a set of lesson sheets in which apparatus is explicitly described and is depicted in almost every case in a manner to make the construction clear. The mere manipulative process of putting together the apparatus from the pieces indicated is a useful discipline to beginners in physics, and Mr. Hammel has been very successful in indi-

cating with brevity and definiteness the forms which each piece is intended to take.

Gage's Laboratory Manual, like some of those which have already been noticed, has been put in the form of a notebook. It is printed on one side of the paper only and there are many entirely blank leaves dispersed upon which to write the results of observations. The work which the author lays out is largely quantitative, and is capable of being carried out precisely in the manner indicated. This is a feature which all experimental work given on paper does not possess. The quality of entire practicability is one which can be given to a book only through the author's prolonged experience as a teacher. The general type of the experiments in Gage's manual is that of those required for entrance to Harvard College; indeed, many of the experiments are taken from the Harvard list.

The direct and official exponent of the Harvard entrance requirement in physics is, however, the new edition of *Hall and Bergen's Text-book of Physics*. This excellent volume has been increased in size from 388 to 596 pages, and there are evidences throughout that the authors have brought their wide teaching experience to bear in the revision. A feature of the revised edition consists of a series of pleasing portraits of scientific worthies from Newton to Faraday, which are placed at appropriate intervals throughout the book. The arrangement of subjects is an unusual one. Light comes between the chapter on Friction and those on the Properties of Solid Bodies. Sound is placed between Heat and Electricity. The space allotted to the last named subject is relatively smaller than in any recent text-book with which we are acquainted.

The amount of work involved in Hall and Bergen's text-book is really very large, much larger than that demanded in the proper completion of any other secondary school book with which we are acquainted. One has the feeling in looking through the experiments that, while these are as far as possible quantitative in nature and well chosen, some further refinement of method and apparatus would be an advantage. Students who are mature enough to do what is demanded of them and able to give the time which so long and comprehensive a course of experiments involves, are likewise capable of some nearer approach to operations of precision. Acquaintance with instruments designed for accurate measurements would greatly increase the respect of the pupil for scientific work and his interest in it. The outlay demanded on the part of the schools would be somewhat increased, but the burden is not one which would be severely felt and the good result is something towards the attainment which the writers of text-books might well use their influence. The mixing, throughout the volume, of British measures with the metric system is to be deplored.

E. L. N.

Applied Mechanics. By JOHN PERRY. With 371 illustrations. New York, D. Van Nostrand Company, 1898.

This is a closely printed octavo of 678 pages and forms a greatly extended edition of a work on the same subject published fifteen years ago. It describes the course of instruction in Applied Mechanics at the Finsbury Technical College, London.

The book contains the substance of the lectures delivered in this course. As a consequence, there is a warmth about it from its chatty, conversational tone that contrasts favorably with the usual formal impersonal treatise. On the other hand, there necessarily enters a certain amount of repetition. Occasionally a repetition not necessary is met with, as for example, on p. 320, where a proposition already demonstrated on p. 182 is again demonstrated.

The book falls naturally into two divisions. The first seven chapters and chapters XXIII. and XXV., forming about one-third of the whole, contain a laboratory treatment of the principles of elementary mechanics. The student is assumed to have had a thorough course in academic mechanics and physics and in the differential and integral calculus. But as "there seems to be as much absence of common sense now in academic persons as there was in the time of Erasmus," the author finds it necessary to have simple experimental tests of the parallelogram of forces, the laws of friction, the mechanical advantage and efficiency of machines, etc., made by the students. In carrying this out, his idea of insisting on the plotting of results obtained on squared paper in order to find the law of dependence of the quantities involved, is worthy of great commendation. It is, in fact, the unique thing in the book.

The remaining two-thirds of the book treats of the application of mechanical principles or of applied mechanics so-called. The subjects taken up are Graphical Statics, Hydraulic Machines, Machinery in General, Materials of Construction, Mechanics of Materials, Metal Arches, Fluids in Motion, and Springs.

In discussing such a variety of subjects in so small a space the treatment must necessarily be brief. But the author has anticipated all objections. He contends that the mechanical principles that an engineer really recollects are very few, and that the "student who depends upon a textbook to give him complete information is not learning to become an engineer."

The treatment proceeds on non-mathematical lines, using laboratory methods as far as possible. Mathematical analysis is in many cases added in smaller type. The position of the author is this: The engineer must recognize that theory is only to guide, the final appeal must be made to experiment. Hence he should study the mathematical treatment of a subject, and get what assistance from it he can. But when a novel thing

has to be done he should be very cautious in using theory. He should pay great attention to what are called "rule of thumb propositions," and remember that these propositions have been reached at considerable cost in trials and failures.

Some of the author's remarks on laboratory work are of interest. Thus "You will learn most when you try to get accuracy of measurement with very rough apparatus. Teachers who do not think will introduce roughness and inaccuracy in the wrong places; they will give you a bad time-keeper perhaps, or an inaccurate scale for measuring distances;" and again, "I find it nearly impossible to get my pupils to believe that rough and rusty old machines, such as screw jacks or hydraulic jacks, which have been long in use, are far more instructive to study than beautiful, specially made, frictionless machines."

The treatment of mechanical units seems quite "academic." Thus, to find kinetic energy, the author uses the formula $mv^2/2$ and for momentum mv, where m denotes mass or, as he prefers to call it, "inertia," and v velocity. He converts weight to mass by dividing by 32.2, and defines the unit of mass as a body whose weight in London is 32.2 lbs.

Now, working engineers use the formula $wv^2/2g$ for energy, and wv/g for momentum. They have no need of the term mass at all, and its introduction only leads to confusion when the gravitation system of units is employed. Of course, the physicist in his laboratory may use any units he pleases, but it is hardly fair for our author to say that he "describes the units of measurement employed practically, not merely in calculation but in thought, by English-speaking people."

In conclusion, we may say that the book is chiefly of value to teachers, for the reason that it presents the methods and opinions of an enthusiastic teacher of Mechanics. Its want of systematic arrangement would be a serious drawback to its use as a text-book.

T. W. WRIGHT.

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Elements of Theoretical Physics. By C. CHRISTIANSEN. Translated by W. F. MAGIE. 8vo, pp. xii+339. London, Macmillan & Co., 1897.

The compact little text-book on theoretical physics by Professor Christiansen, of the University of Copenhagen, has doubtless been familiar to most teachers of physics since its translation into German in 1894. As explained by E. Wiedemann in his preface to the German edition, the need of such a text-book is not due to any lack of books on the subject. Excellent treatises on the various branches of theoretical physics abound, and must be consulted by those intending to make any one branch the subject of special study. But there is a demand for a more brief and

compact presentation of the subject, which shall enable the student to obtain a general view of the whole field in a comparatively short time. With the possible exception of Victor von Lang's Theoretrisçhe Physik, I know of no book which is so nearly an ideal one for this purpose as Christiansen's "Elements." It is a matter for congratulation that Professor Magie's excellent translation now makes the work available for American students.

Perhaps the most surprising thing about the book is the fact that so large an amount of valuable matter can be contained in so small a space. For it must be remembered that practically *all* branches of theoretical physics are discussed. In spite of its brevity, the treatment is by no means superficial. Compactness is obtained, in most cases, rather by a careful selection of what to omit than by any excessive briefness in the presentation of the subjects considered.

Exception may perhaps be taken to the statement above in some instances, notably in the case of the chapter devoted to Thermodynamics. In this chapter only the mathematical skeleton of the subject is presented. The discussion of Entropie and the Second Law is extremely unsatisfactory. Consider, for instance, the following statement: "If, therefore, the body passes from the state A to the state C the integral $\int_A^C \frac{dQ}{T}$ is independent of the path." If the author had called attention to the fact that a non-reversible process cannot be represented by a "path" upon an ordinary diagram, this statement would not be so misleading. But as it is, the student who gives the subject any thought cannot fail to call to mind the case of a gas expanding without doing work, and, therefore, without change in temperature. It requires a careful study of the subject to convince one's self that this case is not in violation of the statement above cited. Not only is there nothing in the text to help the student out of this difficulty, but there are many other statements that are equally puzzling. In the opinion of the reviewer there is no branch of theoretical physics in which clear *physical* ideas are so essential as in thermodynamics. In spite of the formidable analytical expressions that occasionally arise the difficulty of the subject is only in a small part mathematical. A purely mathematical treatment, like that of Christiansen, not only fails in many cases to clear up the real difficulties, but sometimes actually masks them; the text reads so smoothly to one possessing reasonable dexterity in analytical methods that by most readers neither the difficulty nor the beauty of the subject will be properly appreciated.

The discussion of the potential energy of a magnet (pp. 171–173) is perhaps of questionable soundness. The expression for the energy of a magnet when placed in the earth's field (p. 171) is of course incomplete,

since the energy due to the action of the different parts of the magnet on each other is neglected. The most objectionable feature of this discussion is, however, the implied assumption that H is constant throughout the interior of the magnet. As is very common in the mathematical treatment of magnetism the iron considered is supposed to be of that ideal kind, as yet undiscovered, for which the permeability is constant.

With the exception of the chapter on Thermodynamics there are, however, only a few cases where the author's treatment can call for adverse criticism when the aim of the book is fully understood. The subject of mechanics is especially satisfactory. A development of Lagrange's equations in generalized equations might to advantage have been added; but the omission was doubtless forced upon the author by the requirements of brevity. This section illustrates particularly well the author's happy faculty of selection. Those parts of the subject that are fundamental and essential are discussed, briefly, it is true, but with clearness. But it is the essential points *only* that are considered; the section is not encumbered with a mass of material of secondary importance.

The extremely compact form in which the analytical work is printed is occasionally annoying to the reader. Cross references to equations are also in some cases not entirely clear. The work of translation is excellent. There is, it is true, a persistent use of will instead of shall; but usage in such cases depends so much upon individual preference that it is hardly a subject for criticism. In fact, when the author says "we *will*" solve the following problem, the reader is inspired with the feeling that such firm determination must lead to success; and his confidence is usually justified.

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